Popular Astronomy.

Vol. XVIII, No. 4. APRIL, 1910. Whole No. 174

ACHROMATIC AND APOCHROMATIC. COMPARATIVE TESTS.[1]

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FOR POPULAR ASTRONOMY.

§1. *Introduction.*

In the years 1889 and 1899 there appeared two reports on the testing of apochromatic telescope objectives. Dr. C. S. Hastings[2] reported that he considered a 2¾-inch apochromatic which he had made, equal to a 3½-inch or a 3⅜-inch achromatic, a gain of 23 per cent in the latter case. These conclusions were based on visual tests, viz., observations of double stars, and the distances at which a table of logarithms could be read.

Dr. M. Wolf[3] reported on the testing of a 212 mm. apochromatic objective by Dr. M. Pauly of the Zeiss factory. Dr. Wolf tested for chromatic aberration by visual methods, and the following double stars (distances given by Wolf) were separated :

Pair	Distance	Magnitudes
η Cor. Bor.	0″.4	5, 6
μ_2 Bootis	0 .9	7, 8
1 Cor. Bor.	0 .8	6, 7
γ Cor. Bor.	0 .4	4, 7
λ Cassiop.	0 .6	6, 6
μ Cygni	2 .9	4, 5
ζ Herculis	0 .5	3, 7
OΣ 338	0 .7	6½, 6½
Σ 2695	0 .9	6, 6

By a reference to the measures given in Burnham's Catalogue, it seems that the distance of both η Coronae and γ Coronae should be 0″.6 rather than 0″.4, though a single observer or two may have made the latter measure. Thus it seems that the 212 mm. apochromatic probably did not separate any closer pairs than a first class achromatic of the same aperture would

(1) Presented at the meetings of the Astronomical and Astrophysical Society of America, August 25, 1908, and August 19, 1909.

(2) *American Journal of Science and Arts*, vol. 37, 1889.

(3) Zeitschrift f. Instrumentk , 19, 1, 1899.

have separated. ζ Bootis and 52 Arietis, which should not have been separated, could not be.

While the separation of double stars is to be highly appreciated, it is incompetent to use a measure about which there is not agreement as proof of unusual separating power.

My object was to make a comparison for myself, and in addition to visual tests, to get photographic results which should eliminate the eye of the observer and give permanent data which could be studied at leisure.

§ *2. The Objectives.*

Accordingly I purchased two objectives, a Clark achromatic by Lundin, and a Steinheil apochromatic. They were of 2½inch and 2⅜-inch aperture respectively; the Steinheil was a little over 2⅜-inch. Both were of 44 inch focal length. Both were two lens type objectives. The glass of the Clark objective was furnished by Mantois of Paris, that of the Steinheil by the Jena glass works. The chemical composition of the Steinheil objective is as follows :

	SiO_2	Sb_2O_3	$\frac{Mn}{Fe_2}O_3$	K_2O	Na_2O	B_2O_3	CaO	BaO
Crown	55.2		3.7	5.7	7.5		5.9	22.0
Flint	59.9	16.1	2.7	5.1	3.5	12.7		

The makers state that the glasses are permanent. The glasses of the Steinheil were separated by three tin foil leaves; those of the Clark were cemented at least around the rim.

Each maker was informed that the other was to make an objective especially for this test, my object being to get the best possible objective of each type. Mr. Lundin found a defect in the first objective he made when he came to figure it, which defect could not have been discovered before, and he made a second objective which he sent. On seeing the photograph of the knife edge test of the objective first sent by them, Steinheil Söhne were not satisfied with it, and sent a second one. In mechanical finish and as specimens of the optician's skill, both objectives were blameless. As evidence of the accuracy of the Steinheil mechanical work, it may be stated that a collar was made for a forced fit over the end of a telescope tube from measurements of the length of the diameter of the tube, and a strip of paper cut the exact length of the circumference, which I furnished them. The fit was perfect.

Both objectives were provided with adjusting screws, which were found to be indispensable. Both were made to mount consecutively on the same telescope tube. This was sometimes

mounted on my 6½ inch equatorial, driven by clock work, in order to secure every advantage of mounting.

My thanks are due to both makers for the privilege of making the tests. Both are deserving of great credit for their scientific spirit, as contrasted with the spirit of mere commercialism.

§ 3. *The Visual Tests.*

The objectives were tested visually in various ways, but chiefly on double stars. The writer believes in the value of such tests, especially for the one making them, and when in the case of close pairs the measures cited as evidence of unusual separating power represent a consensus of measures. No test was found for the one objective which the other did not stand. In double star tests my aim was generally to use unequal pairs and the lowest available eyepiece that would separate them, as a test of definition. Sometimes a lower eyepiece would have sufficed if I had had one. Both objectives gave perfect star disks, and diffraction rings. The out of focus images were clean and sharp. The apochromatic showed no blue around the disk of Jupiter or Venus, the achromatic scarcely any. In definition and separating power they were equal. Both objectives separated the following pairs:

Pair	Distance	Magnitude	Power
α Lyrae	54″	1, 10.5	37
ι Leonis	2.49	3.9, 7.1	80
70 Ophiuchi	3.2	4.1, 6.1	37
ξ Bootis	2.25	4.7, 6.6	75
ε Bootis	2.8	3, 6.3	75
η Cassiop.	5.9	4, 7.6	37
σ Cassiop.	3.2	5.4, 7.5	37
μ Cygni	2.0	4, 5	75
ζ Orionis	2.6	2, 6	75
β Orionis	9.7	1, 8	37
ϵ_1 Lyrae	3.4	5, 6	37
ϵ_2 Lyrae	2.3	5, 5	75

There was seen with difficulty, if at all, though I feel confident of just seeing the green point,

α Scorpii	3 .1	1, 8	80

Nearly in the same class

μ Librae	1 .7	5.4, 6.3	80

Distinctly elongated in the right direction

λ Ophiuchi	1 .1	4, 6.1	75, 80

Such pairs as α Herculis, Castor, γ Virginis, ζ Aquarii, γ Leonis are no test for either objective being easily separated with a power of 37. The same can be said of Polaris and δ Herculis. Very nearly the same is true of ξ Ursae Majoris.

§ 4. *The Laboratory Tests.*

The laboratory tests applied were the photographic knife edge test, in which the camera is substituted for the eye,[1] and the comparison of intra-and exta- focal spectrograms.[2] The latter were obtained by placing a cap having in a straight line ten small equal and equally spaced circular holes over the objective, in such a way (the telescope being horizontal) that the holes were in a vertical line. The spectroscope slit was placed at equal distances inside and outside of the focus for composite light which was experimentally determined. The telescope of the spectroscope was removed and the camera was substituted for

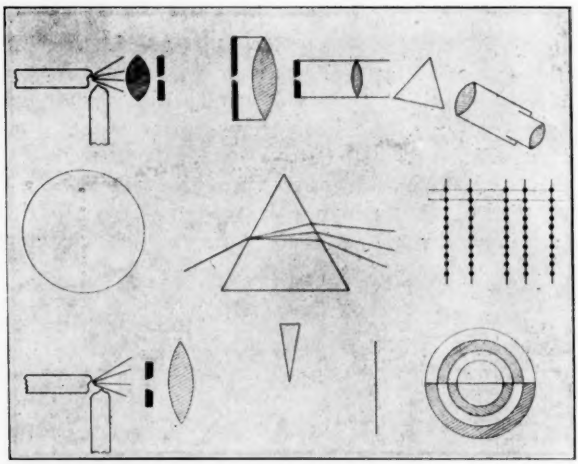


FIGURE 1.—Illustrations of Apparatus.

it. Plates sensitive up to the orange were used. A 90° carbon arc light was placed 120 feet away from the objective, and the light being passed through a pin hole in a metal plate near to the carbon points formed an artificial star. For the spectrograms a solution of calcium salts was applied to the carbons to give a bright line spectrum. Figure 1 represents without being drawn to scale some of the essential parts of the apparatus used in the two tests. It shows diagrammatically a spectrogram and how the photograph of an objective with zones of different curvatures would appear.[3]

(1) J. Hartmann, Sitzungsberichte der k. Preussischen Akademie der Wissenschaften, Dez. 19, 1907.

(2) J. Hartmann, Zeitschrift f wissensch. Photog. 1, 259, 1903; also l. c. 1904. S. Czapski, in Winkelmann's Hand Buch der Physik, Band 6, p. 468.

(3) In testing various objectives one such was found.

For knife edge half of a spectroscope slit was used. The objectives were photographed and then the edge was brought up to the focus from the opposite side, in order to photograph both halves of the objective under the same conditions.

It was found by trial that defects, if present, were visible when the knife edge did not quite coincide with the focus.

Both the two objectives here described were practically free from criticism under this test. Figures 3 and 4 show the results for one side. A little halation is evident; also a slight glare from the edge as was found by rotating the edge. Figure 5 is a focogram of the 6½-inch Clark objective. The extra light and vertical streak are somehow due to the arrangements. No defects of grinding could be detected.

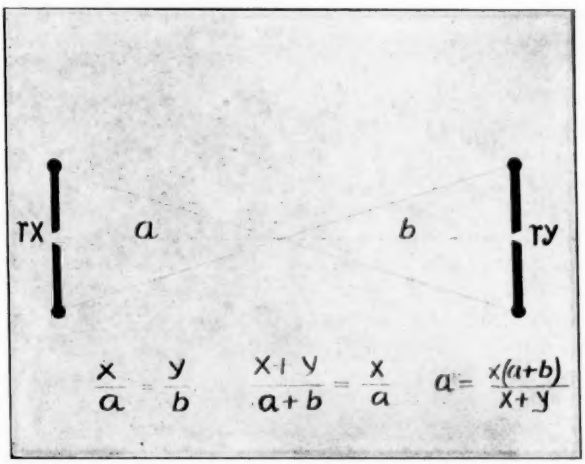


FIG. 2.—Drawing illustrating principle of calculating ordinates of color curves.

For the spectrograms the spectroscope slit was placed 10 cm. in and outside of the focus for composite light for the Steinheil, and 13.9 cm. for the Clark. Figures 6–9 show the spectrograms. It was assumed that the divergence of the rays at the slit was proportional to their divergence at the plate. The writer measured the distances between the images of the different adjacent holes for the different wave lengths with the comparator. Some 3000 settings of the micrometer were involved. The intra-and extra-focal measured distances for the same pair of holes and wave length being called x and y, the actual distances at the slit would be rx, and ry, and the distance of the point of inter-

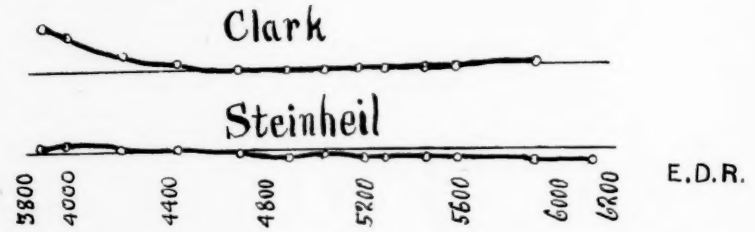
section, or focus for the given wave length and pair of holes, from the internal position of the slit would by similar triangles be given by the formula

$$a = \frac{x(a+b)}{x+y},$$

where $a + b = 20$ cm. for the Steinheil, and 27.8 cm. for the Clark objective. See Figure 2. The average of all the distances a was found for each wave length and pair of holes; 3.9 cm. was subtracted from each value of a for the Clark objective, in order to have the reference point for focal distances 10 cm. inside the focus for composite light for each objective. The values of the wave lengths were taken as abscissas, the values of the a's as ordinates. The following were the results :

Wave Length	Ordinates	
	Clark	Steinheil
3886	10.904 cm.	10.109 cm.
3969	10.749	10.142
4227	10.355	10.105
4455	10.165	10.069
4586	10.070	9.982
4878	10.053	9.895
5041	10.011	9.965
5189	10.007	9.824
5270	9.998	9 824
5350	10.075	9.824
5590	10.066	9.858
5893	10.159	9.724
6150		9.743

From these data, the ordinates in millimeters, I have drawn the color curves as follows :



After this had been done, it occurred to me that it might be objected that this method gave the color curve of the combined system of lenses consisting of the telescope objective, collimator, and camera lenses. Accordingly at my suggestion, the collimator and camera lenses were removed, and the prism set at the angle of minimum deviation was placed considerably inside the focus for composite light which was again determined. The sensitive plate was placed 10 cm. inside and outside of this point. This

was only done for the Steinheil objective. Figures 10 and 11 show the spectrograms. They were measured as before with the following results :

	Prism only. Steinheil
Wave Length	Ordinate
4303	9.82
4355	9.80
4455	9.78
4586	9.85
4878	9.91
5041	9.83
5189	9.79
5270	9.97
5350	9.92
5590	9.96
5893	9.89
6150	9.92

While the curve is not identically the same as before, it is practically of the same character, confirms the previous result, and also shows that the latter method is a feasible one which gives reasonably good results.

From the color curves it will be seen that the secondary spectrum is present in the case of the Clark objective as we should expect, but that for the part of the spectrum for which it was corrected, it shows beautiful correction; while for the Steinheil the secondary spectrum is practically removed, and correction extends throughout the whole spectrum.

In the matter of the laboratory tests I am under special obligation to my colleague Professor Saunders for his expert assistance and generous co-operation. He placed his laboratory at the disposal of the work, and gave freely of his time. He arranged the apparatus and made the plates of the knife edge tests and spectrograms.

To Messrs. Mattoon and Wyman, former students of physics and mathematics at this university, I am indebted for their painstaking calculations of the values of the ordinates of the color curves, which they worked out from the values of x and y which I gave them; also for the drawings for Figures 1 and 2.

To Mr. W. H. Jakway of this city I am indebted for the photographs from the plates and drawings.

§ 5. *Conclusions.*

So far as my investigations go the achromatic and apochromatic are equal in separating power and definition. This conclusion is not due to any inferiority of the Steinheil objective. It is as perfect and faultless as can be. Why the apochromatic should possess greater separating power is not clear, since ac-

cording to physical optics separating power depends on aperture; unless it be claimed that the apochromatic transmits more light than the achromatic and it is all brought to a focus (including the violet) and is therefore all of it efficient, both of which things are virtually equivalent to an increase of aperture. But it is desirable to investigate this theoretical claim further and establish it incontestably if it is true. I should have liked to compare a 6½-inch apochromatic with my Clark 6½-inch achromatic by Lundin, but as I had to undertake the investigation at my own expense, a 6½-inch apochromatic was entirely beyond my means.

I was not able to confirm the before mentioned claim of Hastings. One objective defined as well as the other. According to Hastings the 2⅜-inch apochromatic should have been equal to a 3-inch or a 2.9-inch achromatic. Instead of this nothing done by the 2⅜-inch failed to be done by the 2½-inch achromatic, and *vice versa*. No pair of stars separated or shown by the one failed to be separated or shown by the other. Both are absolutely first class objectives according to my judgment. The Steinheil objective removes the secondary spectrum. Visually the Clark is nearly free from it. Both objectives are perfect products of the optician's skill.

For observing colors of stars the apochromatic should give the more reliable results. The apochromatic being corrected for the whole range of the spectrum, can, without correcting lens or color screen, be used directly for photographic work, and in this, not in the usual visual work, consists its great and decided superiority over the achromatic. It should also be superior in certain spectroscopic work where the secondary spectrum is disturbing.

I wish to record here my admiration of the researches which discovered the glass which by its chemical composition was capable of rationalizing "the irrationality of dispersion."

It may be noted that Steinheil Söhne are ready to furnish apochromatic objectives for refractors up to 15¾-inch aperture.

The type of achromatic objective made by the Clarks and its achievements are too well-known to require further mention. The same type of objective still made by Mr. Lundin meets the most exacting requirements of visual observation where separating power and definition are required.

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THE TILTING OF THE EARTH'S AXIS AND THE GLACIAL EPOCHS.

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FOR POPULAR ASTRONOMY.

The Earth being a spheroid is not a centrobaric body. That is, the resultant of the attractions of a distant body upon all its particles is not a single force acting through its center of inertia, which is its center of figure, but a force acting through this center and also a couple tending to turn the Earth about an axis passing through its center and perpendicular to the plane passing through the axis of the Earth and the distant attracting body. The tendency of this couple is to bring the plane of the equator through the attracting body. In other words if we considered the Earth and Sun alone, and supposed

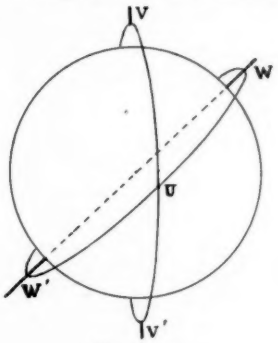

FIGURE 1.

the Earth not to be rotating on its axis, the action of the Sun could be resolved into a force acting at the Earth's center, under which it would describe an orbit according to Kepler's laws, and a couple which would in fact bring the plane of the equator into the plane of the ecliptic. Or rather, if we supposed the plane of the equator to start from a certain position of rest, this plane would oscillate backwards and forwards through the ecliptic, between certain fixed limits, like a pendulum.

But since the Earth is rotating on its axis, it is dynamically a gyroscope, and the attempt to turn a gyroscope in a plane passing through its axis, results in a turning of this axis in a plane perpendicular to the plane of the couple urging it. (Fig. 1)

If, as in Figure I, a globe be suspended by its axis WW' with-

in a ring WUW', and this ring is gimballed within another ring VUV' by pivots at U, the globe is free to move in any direction. If we now set the globe rotating about its axis WW' and at the same time attach a weight at W' this weight will produce a couple which will tend to bring the axis to a vertical position. Since however, the system is a gyroscope, the axis will not turn in the plane of the page, but will turn in a plan perpendicular to the page. The result, as is well-known, is that the axis WW' will circle about the axis VV' at a practically constant angle.

We have here precisely a model of the Earth. If we suppose the axis VV' to be vertical and the attracting body is a horizontal plane passing through the center of the Earth, the moment of the attractional couple due to the Earth's equatorial protuberance will be

$$\frac{3\,S}{D^3}\,(C - A)\sin\delta\cos\delta\,,$$

where S is the mass of the attracting body, D its distance, C and A respectively the moments of inertia about the polar and an equatorial axis of the Earth and δ is the declination of the attracting body. (See Thomson and Tait's Natural Philosophy, Par. 825.)

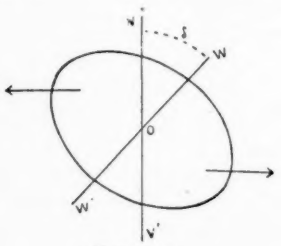

FIGURE 2.

Looking at the Earth from W, which we suppose to be the North Pole, its rotation will be left-handed, while the rotation about VV', or the precession, is right-handed or retrograde. The true gyroscopic nutations or noddings, of the Earth are so minute as to be inappreciable, while the so-called nutations, due to the fact that the plane through the attracting body and the Earth's axis is not always perpendicular to the plane of the orbit, are very small. Consequently, for all practical purposes, we may consider the axis of the Earth to describe a smooth cone about the axis VV', perpendicular to the ecliptic. Actually, on account of the nutations we have mentioned, as well as because of the gyroscopic movement of the Moon's orbit about the pole of the ecliptic, this cone is slightly fluted.

If we designate by $\dot{\psi}$ the angular velocity of the precession, then the angular velocity with which the axis of the Earth turns about its center will be $\dot{\psi} \sin \theta$ and by the gyroscopic law such an angular velocity will produce a couple tending to turn the Earth about an axis through U, and the moment of this couple will be $C\omega\dot{\psi} \sin \theta$ where ω is the angular velocity of the Earth about its polar axis and C is the moment of inertia about this axis. (See article "Gyroscope" in "The Atmosphere," Spon and Chamberlin, New York.)

The direction of this couple is opposite to that of the attractional couple and equal in amount. There is thus equilibrium and as far as these two forces alone are concerned, the Earth moves as it would under the action of no forces, performing a Poinsot motion about the axis VV' as an invariable line. We shall presently see that there is another minute disturbing force which makes such a Poinsot motion impossible.

The attractional couple then imparts a precessional velocity to the axis of the Earth and this precessional velocity in turn gives rise to a gyroscopic couple exactly equal and opposite to the attractional couple. These forces annihilating each other, the axis of the Earth performs its precession at a constant velocity. We can write then

$$C\omega\dot{\psi} \sin \theta = \frac{3S}{D^3} (C - A) \sin \theta \cos \theta. \tag{1}$$

Now it is evident that the joint average precessional effect of both the Sun and Moon can be considered as being effected by a single body in the plane of the ecliptic. It is further evident that the average declination of such an attracting body is half the inclination of the Earth's equator to the ecliptic.

Equation (1) represents the action of the maximum declination of the attracting body. For the average position of an attracting body this equation would become,

$$C\omega\dot{\psi} \sin \theta = \frac{3S}{D^3} (C - A) \sin \frac{\theta}{2} \cos \frac{\theta}{2} = \frac{3S}{2D^3} (C - A) \sin \theta \tag{2}$$

or

$$C\omega\dot{\psi} = \frac{3S}{2D^3} (C - A). \tag{3}$$

This gives the rather remarkable result that no matter what the inclination of the Earth's axis to the ecliptic may be, the precessional period remains unaltered. The precessional period is about 26,000 years.*

* This is a general gyroscopic principle. For instance, a gyroscope actuated by gravity, such as an ordinary top, has a constant precessional period, irrespective of the inclination of its axis. (See Art. "Gyroscope" in "The Atmosphere", Spon and Chamberlain, New York).

Returning to Figure I let us suppose that our globe is a homogeneous sphere, or, if heterogeneous, that its surfaces of equal density are spheres. The moment of inertia is therefore the same, indifferently about any axis. If now while it is rotating about its proper axis WW', we turn it about the axis VV' left-handedly, a gyroscopic couple will be set up, which will shortly bring the axis WW' into coincidence with the axis VV'—W coinciding with V. If we turn it about VV' right-handedly, then W will coincidence with V'. In either case the two initial independent rotations will become fused into one, having a common direction.

If our globe is a spheroid, then the rotation about the axis VV' will set up a centrifugal couple, which it the globe is not rotating about the axis WW' will bring the plane of the equator perpendicular to the axis VV'. If the globe is rotating on its proper axis WW', then by giving a right-handed rotation about VV' it will be possible to arrange it so that the centrifugal couple, which is opposite to the gyroscopic couple, shall be exactly equal to it, and these two couples annihilating each other, the body will perform a Poinsot motion, or will move under the action of no forces. In this case the axis will describe a smooth cone about the axis VV' and the precession will be constant. I have previously (*American Journal of Mathematics*, vol. xxviii, No. 3) called attention to the fact that in a Poinsot motion, or motion of a body under the action of no forces, the gyroscopic and centrifugal couples are in equilibrium.

In the first case we have just considered, there was only a gyroscopic couple at work. In the second case there was a gyroscopic couple and a centrifugal couple. In the case of the Earth we now see clearly that there are three couples at work, viz: a gyroscopic couple, an attractional couple and a centrifugal couple. We have seen that the attractional couple produced a precessional velocity of the Earth's axis which brought into play a gyroscopic couple exactly equal and opposite to the attractional couple. There is still another couple to be considered. The Earth is rotating about an axis perpendicular to the ecliptic with an angular velocity $\dot{\psi}$, which, as we have seen is independent of the inclination of its axis. This velocity, it is true, is exceedingly minute, but it is a disturbing factor which cannot be neglected.

The attractional couple and the gyroscopic couple are in exact equilibrium, the former manufacturing an amount of the latter exactly equal to itself and no more. There is left over

the centrifugal couple due to the precessional rotation of the Earth about the axis VV' which couple tends to bring the axis WW' into coincidence with the axis VV'.

If therefore we start our system from rest at an inclination of the axis θ_o, it is evident that the centrifugal couple will tend to reduce this inclination. Being unopposed, a reduction of the inclination will in fact take place, and this will in no way affect the attractional and gyroscopic couples, since they are in equilibrium at all inclinations. (v. Equation 2).

We may consequently in the investigation of the action of the disturbing centrifugal couple, neglect the two other couples, viz: the attractional and gyroscopic couples, since these two annihilate each other. Our problem then reduces simply to determining the motion of a spheroid rotating about an axis VV' with an angular velocity $\dot{\psi}$ and starting from rest with an inclination of θ_o. The body is to be regarded as having no rotation about its proper axis WW' since we have abolished both the attractional and gyroscopic couples and therefore the axial rotation about WW'. The moment of this centrifugal couple is

$$\frac{M}{5}\dot{\psi}^2 \sin\theta \cos\theta\,(a^2 - c^2) = \dot{\psi}^2 \sin\theta \cos\theta\,(C - A),$$

where a and c are the greatest and least semi-axes respectively of the spheroid, and the spheroid is supposed to be a homogeneous solid. For the two principal moments of inertia are in such a case

$$A = \frac{a^2 + c^2}{5} \cdot M \quad \text{and} \quad C = \frac{2a^2}{5} \cdot M.$$

If the spheroid is heterogeneous but surfaces of equal density are similar and similarly placed spheroids, the moment of the centrifugal couple is still $\dot{\psi}^2 \sin\theta \cos\theta\,(C - A)$.

For let us construct a momental ellipsoid about the fixed point O, and drop a perpendicular from O to the tangent plane to this ellipsoid at I, the point where the axis VV' cuts the momental ellipsoid. Call the angle between this perpendicular and the axis VV', ϕ.

Now

$$\frac{X^2}{\frac{5}{M(a^2 + c^2)}} + \frac{Y^2}{\frac{5}{M(a^2 + c^2)}} + \frac{Z^2}{\frac{5}{2Ma^2}} = 1$$

is the momental ellipsoid about the fixed point O, since the inverse square of any radius vector of this ellipsoid represents the

moment of inertia of the body about this vector as axis. In polar co-ordinates this ellipsoid is

$$\frac{\frac{r^2 \sin^2\theta}{5}}{M(a^2+c^2)} + \frac{\frac{r^2\cos^2\theta}{5}}{2Ma^2} = 1,$$

where θ is the angle that any radius vector makes with the polar axis, and a and c are the greatest and least semi-axes of the body. Hence

$$\frac{1}{r^2} = \frac{M(a^2+c^2)\sin^2\theta}{5} + \frac{2Ma^2\cos^2\theta}{5}$$

is the moment of inertia about a radius vector r making an angle θ with the polar axis of the body. It can be readily deduced that

$$\tan\phi = \frac{\sin\theta\cos\theta\,(a^2+c^2)}{(a^2+c^2)\sin^2\theta + 2a^2\cos^2\theta}.$$

*It can also be readily proved that the moment of the centrifugal couple, G, is $G = T\tan\phi$, when T is the vis viva about the instantaneous axis of rotation VV′, or $T = Mk^2\dot{\psi}^2$, where k is the radius of gyration about the axis VV′.

Now

$$Mk^2 = \frac{1}{r_2} = \frac{M(a^2+c^2)\sin^2\theta}{5} + \frac{2Ma^2\cos^2\theta}{5}.$$

Hence

$$T = \frac{M}{5}\dot{\psi}^2\,[(a^2+c^2)\sin^2\theta + 2a^2\cos^2\theta]$$

and

$$G = \frac{M}{5}\dot{\psi}^2\sin\theta\cos\theta\,(a^2-c^2) = \dot{\psi}^2\sin\theta\cos\theta\,(C-A). \tag{4}$$

This centrifugal couple acts in a plane through the polar axis, so that we may write

$$\frac{M}{5}\dot{\psi}^2\sin\theta\cos\theta\,(a^2-c^2) = -Mk^2\ddot{\theta}, \tag{5}$$

where $\ddot{\theta}$ denotes the angular acceleration.

$$k^2 = \frac{a^2+c^2}{5},$$

since the axis is in the plane of the equator.
Hence

$$\dot{\psi}^2\sin\theta\cos\theta\,(a^2-c^2) = -(a^2+c^2)\ddot{\theta}.$$

Multiplying by $\dot{\theta}$

$$\dot{\theta}\ddot{\theta} = -\frac{a^2-c^2}{a^2+c^2}\dot{\psi}^2\sin\theta\cos\theta\,\dot{\theta}.$$

* See Routh's "Elementary Rigid Dynamics," Art. 260.

Integrating

$$\dot{\theta}^2 = \frac{a^2 - c^2}{a^2 + c^2}\ \dot{\psi}^2\ (\sin^2\theta_\circ - \sin^2\theta),$$

where $\theta_\circ$ is the initial inclination.

Hence

$$\dot{\theta} = \sqrt{\frac{a^2 - c^2}{a^2 + c^2}}\ \dot{\psi}\ \sqrt{\sin^2\theta_\circ - \sin^2\theta}$$

or

$$\frac{d\,\theta}{\sqrt{\sin^2\theta_\circ - \sin^2\theta}} = \sqrt{\frac{a^2 - c^2}{a^2 + c^2}}\ \dot{\psi}\ d\,t$$

or

$$t = \sqrt{\frac{a^2 + c^2}{a^2 - c^2}} \cdot \frac{1}{\dot{\psi}} \int \frac{d\,\theta}{\sqrt{\sin^2\theta_\circ - \sin^2\theta}}\ . \qquad (6)$$

This is an elliptic integral which may be integrated as follows.

Let us use the auxiliary angle χ, such that $\sin\theta_\circ \sin\chi = \sin\theta$.

$$\therefore\ \sin\theta_\circ \cos\chi\, d\,\chi = \cos\theta\, d\,\theta \text{ and } \cos\theta = \sqrt{1 - \sin^2\theta_\circ \sin^2\chi}\ .$$

It will be noted that when θ becomes $\theta_\circ$, $\chi = \frac{\pi}{2}$.

Substituting we have

$$\frac{d\,\theta}{\sqrt{\sin^2\theta_\circ - \sin^2\theta}} = \frac{d\,\chi}{\sqrt{1 - \sin^2\theta_\circ \sin^2\chi}}\ .$$

Developed in series

$$\frac{1}{\sqrt{1 - \sin^2\theta_\circ \sin^2\chi}} = 1 + \frac{1}{2}\sin^2\theta_\circ \sin^2\chi + \frac{1.3}{2.4}\sin^4\theta_\circ \sin^4\chi + \frac{1.3.5}{2.4.6}\sin^6\theta_\circ \sin^6\chi, \text{ etc.}$$

Now (Williamson's Integral Calculus, Art. 93)

$$\int_0^{\frac{\pi}{2}} \sin^{2n}\chi\ d\,\chi = \frac{1.3.5\ \ldots\ 2n-1}{2.4.6\ \ldots\ 2n} \cdot \frac{\pi}{2}\ .$$

$$\therefore \quad \int_0^{\frac{\pi}{2}} \frac{d\,\theta}{\sqrt{\sin^2\theta_\circ - \sin^2\theta}} = \frac{\pi}{2}\left[1 + \left(\frac{1}{2}\right)^2 \sin^2\theta_\circ + \left(\frac{1.3}{2.4}\right)^2 \sin^4\theta_\circ + \left(\frac{1.3.5}{2.4.6}\right)^2 \sin^6\theta_\circ + \ldots\right]$$

Hence, from (6), we have as the time of one complete swing,—that is from a position of rest at $\theta_\circ$, through $\theta = 0$ to $\theta = \theta_\circ$ and back again through zero to $\theta_\circ$—

$$T = \sqrt{\frac{a^2 + c^2}{a^2 - c^2}} \cdot \frac{2\pi}{\dot{\psi}}\left[1 + \left(\frac{1}{2}\right)^2 \sin^2\theta_\circ + \left(\frac{1.3}{2.4}\right)^2 \sin^4\theta_\circ + \left(\frac{1.3.5}{2.4.6}\right)^2 \sin^6\theta_\circ + \ldots\right]. \qquad (7)$$

Taking the precessional period as 26,000 years and a year as the unit of time,

$$\dot{\psi} = \frac{2\pi}{26{,}000}.$$

Since

$$\sqrt{\frac{a^2+c^2}{a^2-c^2}} = \sqrt{\frac{A}{C-A}}, \text{ we can write (7)}$$

$$T = \sqrt{\frac{A}{C-A}} \cdot \frac{2\pi}{\dot{\psi}} \left[1 + \left(\frac{1}{2}\right)^2 \sin^2\theta_0 + \left(\frac{1.3}{2.4}\right)^2 \sin^4\theta_0 + \left(\frac{1.3.5}{2.4.6}\right)^2 \sin^6\theta_0 + \ldots \right]. \quad (8)$$

Now the most probable value of $\frac{C-A}{C}$ that we as yet possess, is according to Routh, .0032. Whence

$$\frac{A}{C-A} = 311.5 \text{ and } \sqrt{\frac{A}{C-A}} = 17.65.$$

Hence (8) is

$$T = 26{,}000 \times 17.65 \left[1 + \left(\frac{1}{2}\right)^2 \sin^2\theta_0 + \left(\frac{1.3}{2.4}\right)^2 \sin^4\theta_0 + \ldots \right]. \quad (9)$$

If we suppose θ_0 to be 30°, the value of the parenthesis is approximately 1.071. Hence it follows that if the extreme inclination of the Earth's axis is 30°, the time for a complete swing would be 526,377 years, or the time from an extreme inclination to a position of perpendicularity to the ecliptic would be 131,594 years.

Not knowing from what exact inclination the Earth began its fall, we do not know the value of θ_0, but whatever the initial angle, the total period will not vary greatly. If the extreme inclination had been 35°, the period would be greater by a considerable number of years, but relatively to the period for 30° not very much greater.

If the above reasoning is correct—and that must be left to the mature consideration of mathematicians—we must follow it to its legitimate conclusions. Owing to the small disturbing cause which we have pointed out and which does not seem to be compensated in any way, we are forced to the conclusion that the Earth's axis cannot maintain its inclination to the ecliptic permanently, but swings slowly through a position of perpendicularity in an extremely long period.

The author has no desire to add another glacial theory to the already large existing number. There are enough of these ex-

tremely forced and often preposterous explanations, and the end is not yet.*

A dynamical deduction is not a theory and it is easily possible to determine whether or not the laws of dynamics have been correctly used. If they have been correctly used, then there is no gainsaying the conclusions, for the laws of dynamics are facts not theories.

We must now consider some of the objective facts. The remains of a temperate or even sub-tropical flora are found as near to the pole as 83°. From the species we can deduce the fact that the temperatures at those extreme northern points must once have been not less than 60° Fahrenheit and possibly 70°. The temperature at the Earth's surface depends wholly upon the Sun. If the Earth's axis at that remote period had had its present inclination, then these plants must have survived, at half-yearly periods, six months of darkness, and snow and ice. It is hardly necessary to elaborate the argument further, for if reason is of any avail whatever, we may say most positively that the fact that sub-tropical plants once grew near the pole, proves conclusively that the pole at that time stood perpendicular, or nearly perpendicular, to the ecliptic. There is no escaping it. This fact alone, when pointed out to the mathematician, physicist or astronomer, must cause him at once to admit that the pole was then perpendicular and to wonder how it came to be so.

Professor Tyndal's "Greenhouse Theory" was most unfortunate and has worked great mischief among men of lesser calibre. It is wholly vitiated by its neglect of the important element of time. The climates of the Earth have changed and are still changing. The process is, no doubt, a very slow one, but perhaps appreciable even in historic times. While no great stress can be laid upon it, it is nevertheless a fact that to-day it never snows in Rome. This was not so 1900 years ago. Witness the lines of Horace :

> "Jam satis terris nivis atque dirae
> Grandinis misit Pater et rubente
> Jaculatus arces."

The ice sheet of the last glacial epoch had its greatest extension variously estimated at from 8,000 to 40,000 years ago. There is great probability that it was less than the last number. All the evidence at hand, including the lessening of the present glacial areas, points to the conclusion that the pole is now swinging to a position of perpendicularity. It will reach this

* One of the latest is : "The Zonal Belt Hypothesis," 1908.

position in something like 100,000 years.

The glacial epochs, or times of maximum glaciation, are, according to our present calculation, separated by periods of about 260,000 years. There is some reason for believing that the close of the Cretaceous Age was marked by a glacial epoch and there is tolerable certainty that the Paleozoic and Mesozoic eras were divided by such a period in the Permian Age. Farther back there is considerable evidence of such a period in Cambrian or Pre-Cambrian time. There thus seems to be rather definite evidence of at least four glacial epochs. Some geologists are of the opinion that there may have been more. Thus an epoch before the Cambrian, or a pre-archaeon glacial period, is hinted at, and some have thought that there was such a period in the Carboniferous—possibly two such periods. If we are finally led to conclude that the interval between these periods could not have been more than 260,000 years, or roughly 300,000 years, and if we could definitely locate the succession of such periods in the geological record, we should have a positive measure of the elapsed time. Thus, if we were certain that the Paleozoic, Mesozoic and Tertiary eras were divided by glacial epochs, we might with some certainty fix the duration of each of those eras as roughly 300,000 years. This is a considerable reduction on previous estimates which have run into the millions.

We have not as yet spoken of the motion of the plane of the ecliptic, which would exercise a small influence on the inclination of the Earth's axis. Before taking this up, we may mention a few problems in relation to glaciation which are still awaiting a complete solution. It would be difficult, but still quite possible to work out the average surface temperatures of the Earth and the amount of glaciation for any inclination of the Earth's axis. With a perpendicular axis, there could, of course, be no glaciation. With no ice, the enormous pressure of the aqueous vapor of the atmosphere would be equably diffused over the whole Earth's surface. During the height of a glacial period, there would be but little aqueous vapor in the atmosphere nearly all of the previous content of the atmosphere being converted into ice and its pressure concentrated about the poles.

Professor Darwin has shown that a heavy weight laid upon even a rocky bottom causes a measurable dimple in its vicinity. The effect of the heavy ice masses, therefore, would be to change very sensibly the shape of the Earth, diminishing the polar diameter and causing the equator to bulge out. This would lead to extensive fracturing of the Earth's crust—the word crust

being used without prejudice—and to the extrusion of much lava. As a matter of fact the periods of extensive glaciation were periods of great lava out-flows. We should thus have some definite dynamical basis by which to account for the extensive risings and sinkings of the surface in times past in contradistinction to the divine manner in which geologists raise and depress whole continents whenever the inclination strikes them.

The perturbations of the planets on each other cause slight variations in the elements of their orbits. Their eccentricities, the direction of their major axes and the inclinations of their planes vary slightly in periods covering usually great intervals of time. The theoretical calculation of these actions is extremely complex and the numerical results somewhat uncertain. Lagrange deduced the famous law, $\Sigma m \sqrt{a} \tan^2 i = C$, which in words is, that the sum of the masses of the planets, into the square roots of their major axes, into the square of the inclinations of their orbits to a fixed plane of reference, always remains constant. It has been expressed by saying that there is a certain inclination fund in the solar system which never varies. Laplace took as the plane of reference the astronomical invariable plane based simply upon the revolution of the planets in their orbits, wholly neglecting their rotations on their axes and also the rotation of the Sun on its axis. In Laplace's time the masses of the planets as well as their periods of rotation were not very accurately known. By considering the rotations of the planets and Sun, it is possible to derive the true dynamical invariable plane of the solar system, and this would doubtless differ but little from Laplace's astronomical invariable plane. Lagrange's law was deduced on the supposition that all the members of the solar system have their motion in the same sense. Since his time certain bodies have been discovered which move in a retrograde direction, and this somewhat vitiates his law.

Young states, "The obliquity of the ecliptic is 24′ less than it was 2,000 years ago and it is decreasing ½″ a year. It is computed that this decrease will continue for about 15,000 years, reducing the obliquity to 22¼° when it will increase again. Lagrange states that the whole change can never exceed 1° 2′ on each side of the mean."

W. W. R. Ball says "Leverrier calculated within much narrower limits than Laplace had done (same as Lagrange) the extent within which the inclinations and eccentricities of the planetary orbits vary." I have been unable to find what Lever-

rier's limits were, but they are evidently considerably less than Lagrange's estimate. The uncertainty attending these calculations, owing to their enormous complexity, is illustrated by the following well-known case. Laplace calculated that, owing to the decreasing ellipticity of the Earth's orbit, the Moon would gain 10″ of arc in a century. This was exactly the amount of the Moon's secular acceleration and thus the long-standing mystery was thought to have been solved.

Adams afterwards showed that Laplace had neglected certain terms in an expression and that the gain from the Earth's decreasing ellipticity could not be more than 6″ or about half Laplace's estimate. Plana and Pontécoulant denied this, but Delaunay and Cayley confirmed it.

A. Berry says, "The series of propositions as to the stability of the solar system established by Lagrange and Laplace have been confirmed and extended by later mathematicians such as Poisson and Leverrier; but their claims to give information as to the condition of the actual solar system at an indefinitely distant future time receives much less assent now than formerly. The general trend of scientific thought has been towards the fuller recognition of the merely approximate character of even the best ascertained portions of our knowledge. In the case of the particular propositions in question the progress of astronomy and physics has thrown a good deal of emphasis on some of the points in which the assumptions of Lagrange and Laplace are not satisfied by the actual solar system."

If the orbits of the planets were circular, there would be absolutely no motion of their planes. As, however, they deviate very slightly from circles, there is a very small, and a very slow oscillation of these planes, but the motion is not regular and in no way analogous to the swing of a pendulum. These motions are confined within very narrow limits—in this case of the Earth probably within 1°, perhaps much less—and they cannot be said to have properly any period. To the main swing of the Earth's axis, due to the centrifugal couple which we have already investigated, the very slight shifting of the plane of the ecliptic would be therefore superadded as a perturbation. It has been definitely determined by observation that the obliquity of the ecliptic is decreasing about a ½″ a year, or more accurately 48.2″ a century. This has been explained as due to the motion of the plane of the ecliptic. A part of this decrease is in all probability due to the motion of the ecliptic, how much it is difficult to say, but the decrease is a progressive one, and will continue

until the equator and ecliptic coincide.

What the inclination of the Earth's axis in past times has been is somewhat uncertain from the lack of accurate measurements. The Chinese claim (Ball) to have found the obliquity 23° 52′ in 1100 B. C. It was probably not far from this value at that time, but we can give no great credence to the Chinese observations.

Eratosthenes (250 B. C.) found the obliquity to be 23° 51′ 20″. The fact that he took pains to give the number of seconds leads us to suppose that he, at least, believed he could measure the angle within such limits of accuracy.

A. Berry says, "Hipparchus (120 B. C.) measured the obliquity of the ecliptic, as several of his predecessors had done, and as the results indicated no appreciable change, concluded that the inclination did not vary." Hipparchus' result was 23° 51′. Ball states that at that time (120 B. C.) the obliquity was 23° 46′ and Berry says it was 23° 44′. On the whole I should prefer Hipparchus' estimate of 23° 51′. It seems quite possible that these early Greek astronomers may have been able to measure an angle within one minute. Hipparchus was seeking earnestly to see if there was any discrepancy, even the slightest, between the earlier measurements and his own. He found none within one minute and concluded the value had not changed.

Ball and Berry base their estimate upon Pontécoulant's calculation that the obliquity had decreased 16′ since the Christian era. Pontécoulant (1795—1871) firmly believed that the world was created about 4,000 years before the birth of Christ and pointed to the fact as one of considerable significance that, according to his calculations, at the time of the creation, the line of the equinoctial coincided with the apse of the Earth's orbit. (Théorie Planetaire.) From such a symmetrical position did the Almighty start the Earth upon its travels!

If we ignore the motion of the ecliptic which after all may be so slight as to be insensible, and if the value of the obliquity was 23° 51′, 120 B. C., we can by Equation (9) calculate the extreme inclination possible, as well as the change of inclination between any two periods. I have not as yet carried out this calculation, deeming it better to wait until the basis upon which we stand is better determined. Roughly, however, we may say that the couple due to the centrifugal forces would account for the greater part of the observed decrease of the obliquity of the ecliptic.

Brookline, Mass.

AN HISTORICAL EXAMINATION OF THE CONNECTION OF CALIXTUS III WITH HALLEY'S COMET.

WILLIAM F. RIGGE

For Popular Astronomy.

There has just appeared from the Vatican Press a large-sized pamphlet of forty pages, which deserves to be regarded as the final word concerning the connection between Calixtus III and Halley's comet. It is entitled "Calixte III et la Comète de Halley," and was written by Father J. Stein, assistant to Father Hagen at the Vatican Observatory.

The author has had access to every existing document, and has treated his subject so exhaustively from an historical point of view, that one cannot withhold the conviction that he has settled the question for all time, and that future writers can do nothing better than consult the present pamphlet. No further apology therefore is needed for presenting this short review of Father Stein's work to all those that are desirous of knowing the exact facts of the case.

The author begins by placing before us a few historical facts. They are in brief that the Turks had captured Constantinople on May 29, 1453, and that the reigning Pontiff Nicholas V had called upon all Christian kings to organize a crusade against the Turks. As he died shortly after, Calixtus III ascended the papal throne on April 8, 1455, and at once vigorously prosecuted the work of his predecessor. But as his words fell upon deaf ears, he addressed himself to the faithful at large and issued a solemn bull on June 29, 1456, in which he ordered that in every church throughout the world, every day between Nones and Vespers, one or more loud bells should be rung three times, in the same way as is done for the Angelus, and that then everybody should say the "Our Father" and the "Hail Mary" three times. In addition he ordained that on the first Sunday of every month there should be general processions, in which everybody should take part, and that at these a sermon should be preached to confirm the hearers in the faith, to induce them to practice patience in their adversity, and to do penance, and finally that the hearers should be told of the cruelty of the Turks and of the harm they had done the Christians and were trying to do now, and that all should offer up their prayers and vows to God against them.

All the important parts of this celebrated bull are given in the original Latin, and it is easy to see that there is no reference of any kind to a comet.

Halley's comet had appeared a few weeks before the promulgation of the bull. It had been seen in Italy for the first time at the beginning of that same month of June. During the latter half of the month it was visible in its greatest splendor for more than three hours after sunset. The tail is said to have been sixty degrees long and of a golden color, and to have resembled a waving flame.

The first Sunday of July, 1456, fell on the fourth day of the month. On that day the first solemn procession ordered by the bull was held in Rome, and also on that same day the Turks began the siege of Belgrade.

The comet was still visible, but disappeared from view a few days after Toscanelli's last observation of it on the 8th. On the 14th there was a naval combat on the Danube, in which the Christians gained some advantage over the Turks. But on the 21st and 22nd the Turks were routed in a decisive battle. The comet at that time was no longer visible to the naked eye, and could not therefore have influenced the combatants.

The news of the great victory reached Rome on August 6, the feast of the Transfiguration. This feast, which had been celebrated only in a few places, was on the first anniversary of that day, that is on August 6, 1457, for that reason made obligatory for the universal church. And on its next anniversary, August 6, 1458, Pope Calixtus III died after a reign of only three years and four months.

These being the facts, the author then recounts the comet story as it appears in modern writers. He quotes from Laplace, Daru, Arago, Littrow, Smyth, Grant, Babinet, Chambers, Flammarion, Draper, Newcomb, Jamin, White, Henkel, Dean, and the *Scientific American*. It is very interesting to read the many variations that these writers put upon the story. Some of these the author succeeds in tracing to previous writers, whose words have been marvellously misstated or added to.

He then examines all the contemporary historians, Saint Antoninus, Aeneas Sylvius, Saint John Capistran, ten Italian and seven foreign chroniclers, not a single one of whom connects Calixtus III in any way with Halley's comet, although they all more or less mention the bull, the prayers, the processions, the Turks, the comet, and other items.

The origin of the bull-and-comet myth is finally traced to

Platina, who was commissioned in 1471 by Sixtus IV to write his History of the Popes, and who after having finished it in 1475 was made librarian (not archivist) at the Vatican. He died in 1481.

As to Platina's character as an historian, Gregorovius says that "he wrote with ease and elegance. But his work is without historical foundation and without mental acumen, and is only an agreeable handbook, in which classic biography is seen to have been the exemplar." Pastor calls his life of Paul II a veritable historical caricature.

Platina was a contemporary of Calixtus III. And as there seems to be no reason for questioning his statements in the present instance, his narrative is not without authority. The authenticity of the text is beyond dispute, because the magnificent manuscript of his work, which he personally remitted to Pope Sixtus IV, is perfectly preserved in the Vatican library. As it is the only contemporary source that connects the pope with the comet in any way, the famous passage is given by a faithful photographic reproduction of Platina's original (Latin) manuscript. A literal (French) translation is then added, which is as follows (in English).

"A hairy and reddish comet appearing for several days: as the mathematicians predicted an enormous pest, a drought, some great calamity: Calixtus, in order to avert the wrath of God, ordered processions [the Latin *supplicationes* is not the English *supplications*] to be held on certain days: in order that if any evils were impending, He would turn them wholly on the Turks, the enemies of the Christian name. He ordered also, in order to move God by assiduous prayers, that at noon a signal should be given by bells to all the faithful, that by their prayers they might come to the aid of those who were unremittingly fighting the Turks."

While the main facts of this statement are substantially true, it is the motive—the dread of the comet—actuating Calixtus, that is at issue.

The author in commenting upon this passage proves that the noon signals given by the bells have no connection with the comet, first, because no mention is made of the comet in the bull which orders these signals, and secondly, because no contemporary writer brings the two together. He then takes up the question of the processions. If these were to be held on account of the comet, then there must have been two processions, one on account of the comet as Platina says, and the other on

account of the Turks as the bull orders. If so, why then does Platina not mention the processions ordered in the bull, which were celebrated with such extraordinary solemnity and such large gatherings of the people, that all contemporary historians speak of them. Why then does not Saint Antoninus, to whom as archbishop of Florence, all pontifical documents were addressed personally, mention the comet processions, seeing that he speaks of the other processions and of the comet. Why do all contemporary historians who mention the comet or the processions or both, remain so silent in regard to the prayers against the comet? That the processions ordered in the bull should have been conjointly directed against both the comet and the Turks, or that the historical testimony connecting the two has been lost, are gratuitous assumptions which can be discussed only after they have been proved.

The Platina vein is then followed in the writers of the succeeding centuries, such as Calvisius, Spondanus, Lubienietski, Fleury continued by Fabre, Bruys, Laplace, and Arago, and the growth of the comet story is noted. All authors are quoted in the original, and there are numberless foot-notes in small type full of collateral information.

Citations from the greatest astronomers, from Calixtus's contemporary Toscanelli, and even from Newton and Arago, prove that comets may do harm to the Earth. Even in our enlightened twentieth century, at the present return of Halley's comet in 1910, Dr. Smart, speaking of our passage through the comet's tail on May 19, says "that matter thrown off by the comet at the node may enter our atmosphere, in which case we must hope that cyanogen, which so often appears in cometary spectra, may not be inconveniently in evidence." Why then should we blame the astronomers of the fifteenth century for their dread of comets?

The opposite view, that comets are absolutely harmless, is also proved by a large number of quotations, a very interesting one being found in a letter of Father Kircher's in Rome, January 31, 1665, in which he speaks of a conversation he had with Pope Alexander VII on occasion of the great comet of 1664, and says that the pope laughed at the fears and other unfavorable predictions of astronomers and considered them as so many imaginations.

In conclusion let me transcribe completely the author's own recapitulation.

"1. Among the 101 volumes in folio of the *Regesti* pertaining

the pontificate of Calixtus III (1455-1458), that are preserved in the Archives of the Vatican, not a single one of the documents contained in them makes the slightest allusion to Halley's comet.

2. In the authentic bull of June 29, 1456, which is to be found amongst these official acts, the pope, for the purpose of imploring the help of God against the Turks, decrees solemn processions and orders prayers, the signal for which is to be given about noon by the sound of bells.

3. The fact is incontestable that this bull was promulgated a few weeks after the appearance of Halley's comet, after the astronomers had already on their part published sinister prognostications, and that the processions ordered by the bull were held in the city of Rome for the first time on July 4, when the comet was still visible.

4. It is no less incontestable that the religious ordinances prescribed by the pope had no connection whatever with the appearance of the comet. The testimony of contemporary authors, at whose head stands Saint Antoninus, archbishop of Florence (+1459), confirms this truth, and the authentic text of the bull itself is its most convincing proof.

In spite of this historic fact, however, the testimony of the humanist Platina, librarian of the Vatican from 1475 to 1481, has been invoked and is still often invoked.

5. Platina, in his work *Vitae Pontificum* (completed about the beginning of 1475), mentions the processions (*supplicationes*) and the noon prayers exactly as they are to be found ordered in the bull; but when he says that the gloomy judgments of the astronomers were the motive of the pontiff's orders, he draws upon his own imagination.

6. In the judgment of writers like Gregorovius, Burckhardt and Pastor the authority of Platina as an historian is such that his testimony ought to be accepted with great caution, even when there is no positive proof of error in his statements.

7. His narative has nevertheless remained for many writers the only historical source of this fact, while true documents have been neglected. Calvisius and Fabre, the continuator of Fleury, have more than all others contributed towards the acceptance of the fable inserted in the *Vitae Pontificum*.

8. Amongst astronomers Laplace seems to have been the first to make use of the phrase "exorcise the comet" ("conjurer la comète"). Arago in 1832 speaks of an excommunication, and from that time on to our own day, hardly has the hairy star begun to put in its appearance but we hear of exorcisms and

anathemas. In raillery of this sort the prize will readily be conceded to Babinet: he makes the Franciscans exorcise the comet during the battle of Belgrade.

9. The comet had disappeared several days before the battle of Belgrade; it could not therefore in any way frighten the combatants.

10. The assertion that the comet was universally considered as a presage of the defeat of the Christians by the Turks, is entirely without foundation. Several contemporary authors even say the very contrary. On the whole the terror caused by the appearance of the year 1456 has been too much exaggerated.

11. A certain dread of comets was justifiable at that period, when the cosmic nature of these bodies and the laws of their motion were unknown, and the opinions of eminent savants like Toscanelli were not of a nature to calm the imagination.

12. A little historical criticism never does any harm, even in writing the history of comets."

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HAS THE MOON A ROTATORY MOVEMENT?

Pt. 1 by E. F. del RINCON
Pt. 2 by ALFRED J. MILLER

FOR POPULAR ASTRONOMY.

PART 1.

In many books of astronomy which I have had occasion to read, I have seen affirmed in clear and categoric manner that our satellite has a movement of rotation. Nor have I encountered one astronomer who may be determined in favor of the contrary opinion. Even the widely read Flammarion, one of those who have contributed most with their elegant style to the popularization of astronomical science, maintains the same thesis. Perhaps there are some who withhold themselves from this general opinion, but I do not know them.

Is it that the subject has all the marks of axiomatic truth? So evident is that rotary motion of the Moon that not the slightest shadow of doubt can obtain in any thinking mind? I have thought and re-thought much upon it, and frankly I have not found that evidence in any part. Moreover, I have always believed that what those astronomers considered a movement

of rotation, was precisely *an inevitable effect of the lack of the same.*

They who assert that the Moon has a rotatory motion, reason in the following manner—"The Moon gyrates about the Earth, presenting always the same hemisphere; hence it is evident that, at the conclusion of her translation, she will also have made one turn upon herself. Therefore, she has a movement of rotation."

The *fact* is certain—irrefutable. The Moon, in each one of her sidereal days, excutes one turn upon her axis. But can this with propriety be called a *movement of rotation*? I think not. Moreover I judge that the Moon makes this turn on herself, precisely because she has not the said motion.

I will endeavor to explain. In my judgment, movement of rotation of an heavenly body, properly speaking, is that by which the latter gyrates upon its axis *through its own power* (*i. e. in consequence of a particular force existent in it* and foreign to the other cosmic forces), and therefore independent of the other sidereal movements. In the Moon that particular revolving force does not exist, and for this reason she always presents to us the same hemishere. She is imprisoned by terrestrial attraction, which holds her within the orbit, and this attraction acts in a more powerful manner upon the related hemisphere than upon the rest of the mass; and, as she *has no power* to gyrate and extricate herself from this magnetic yoke which compels her to keep constantly looking on the Earth as though hypnotized by it, it follows that she always shows us the same face; and for this cause, that is, *for her want of gyratory force*, she executes one turn upon her axis in each one of her sidereal days.

An example will render the demonstration clearer. If we elevate a balloon, and suppose that it makes one complete turn with the Earth, at the end of its journey it will also have executed a turn about itself. And do we on this account say that the balloon elevated by us has a movement of rotation *properly speaking*? On the contrary, it turns upon itself *because it has no revolving force*; and as the terrestrial attraction is greater in the basket than in the upper part of the balloon the former will necessarily be always nearer and looking at the surface of the Earth, and at the end of its trip the whole will have made a complete turn about itself.

Besides, is it not odd, extraordinarily odd, that the Moon spends exactly the same time in her rotation and in her revolu-

tion around the Earth? Is it not *too marvellous* that she should occupy the same hours, minutes and even seconds in both motions? Odd and very odd it is; and this alone ought to be enough to convince those who profess the opinion which I try to refute. In what other heavenly body does a similar phenomenon occur? The Earth makes somewhat more than 366 rotations during each revolution; Mercury, 87 and two-thirds; Venus, 231; Mars, 669 and two-thirds; Jupiter, 10,456; and Saturn, 25,070; those of Uranus and Neptune being still difficult to determine with precision. The Moon, instead, spends 27 days, 7 hours, 43 minutes and 11 seconds in her revolution around our globe; and the same, *exactly the same*, in her rotation. And it may not be objected that the Moon, during her year, effects approximately 13 rotations; because what is necessary to attend to is her movement around our planet, which is her center, as the Sun is of the Earth and other planets of our system. Although the Moon revolves around the Sun, it is not *through her will* but *necessarily*—in tow—dragged by the Earth. And neither may it be objected that, to judge by *slight indications*, the same thing occurs in some of the satellites of Jupiter and Saturn as in the Moon, since that does not pass being a *supposition*; and, though it should happen thus, such fact would only demonstrate that the said satellites were also deprived of gyratory force.

What would befall the Earth with respect to its motions, if its force of rotation were to be extinguished? Surely what happens to the Moon in relation to us would happen to it in relation to the Sun. It would continue gyrating around him; but exhibiting always the same hemisphere, because the solar attraction would not permit it *to change its attitude* with respect to the Sun. [?] In that case it is true we would have no succession of days and nights on our planet, such as we now have and as also occurs on the Moon; and the hemisphere which should look upon the Sun would be constantly illuminated by him, whilst the opposite would find itself sunk in a night without end. Besides this, there would necessarily be the difference that the Sun, which is the center of our orbit, is also our source of light; while the Earth, center of the lunar orbit, is a body without light of its own.

In brief: rotation *as a fact* exists in the Moon: as a *movement properly so-called*, that is, *produced by a peculiar force existent in her* (which is its real acceptation), it can be affirmed, without danger of falling in error, that it does not exist.

Did the Moon have this movement in former times? Perhaps; and it is possible that the librations which are observed in her, and which permit us to see some of her opposite hemisphere, may be due to the last efforts of that rotary energy, now near death, which still struggles to disengage itself from the terrestrial attraction which has destroyed it.

PART 2.

Part one is a translation of an article in *Nuveo Mundo* for December 16, 1909. It contains one manifest error. Were the gyral motion of the Earth entirely extinguished, the latter would not continually present one hemisphere to the Sun. It would present all sides to the Sun in turn. The day would equal the year in length. Perhaps, however, Señor E. F. del Rincón will object that in this case a small amount of rotary movement would remain—an amount just sufficient to turn the Earth once on its axis in a year. The article is an excellent argument against the theory that the Moon rotates.

This difference in opinion is by no means a new one. I am not prepared to state how old it is, but the following passage occurs in Kant's Critique of Pure Reason:—"Herr von Mairan regarded the controversy between two celebrated astronomers, which arose from a similar difficulty as to the choice of a proper standpoint, as a phenomenon of sufficient importance to warrant a separate treatise on the subject. The one concluded: *the Moon revolves on its own axis*, because it constantly presents the same side to the Earth; the other declared that *the Moon does not revolve on its own axis*, for the same reason. Both conclusions were perfectly correct, according to the point of view from which the motions of the Moon were considered." Trans. Dia., Bk. 2, ch. 2, fourth antinomy.

Still, notwithstanding the logical possibility of two non-contradictory conclusions drawn from different points of view, it is certain that only one can be actually true. Either the Moon rotates or it does not. And, although the thesis has been ably contested, the purpose of this part is a renewed defense of it.

In the first place, the exact agreement between the periods of revolution and rotation of the Moon should be inquired into. There is a bare possibility of the relation being fortuitous, but this explanation is rightly looked upon with doubt. "It would be against all probability to suppose that these two motions had been at their origin perfectly equal," declared Laplace. Certain astronomers accordingly have imagined that there was

a slight disagreement in the beginning, and that the attraction of the Earth eventually brought both periods into complete agreement. But, in order to enable terrestrial attraction to produce such an effect, the discrepancy must necessarily have been so very slight that its presupposition would likewise be contrary to probability. Newcomb gives what is no doubt the only satisfactory explanation of the phenomenon. Very likely the Moon, while yet in a fluid state, turned much faster or slower than at present. Great tides were produced by the influence of the Earth, and they on the one hand retarded the Moon's rotation or on the other hand accelerated it, until finally the periods of rotation and revolution became of equal length.

Now is this gyratory motion, this undeniable *fact*, a movement of rotation in a strict sense? Is it a consequence of her own power—a result of energy existent within her? Assuredly it is. Were the Earth during any day to vanish into non-existence at the moment the Moon would be farthest or nearest the Sun, and were the speed of the Moon at the same time altered so she would describe an orbit somewhat larger or smaller than the Earth's, the Moon would not only revolve around the Sun but would rotate on her axis as well. Therefore she has a movement of rotation *per se*, or properly so-called, for whence would she have obtained axial motion in the absence of a previous possession of the same ?

The assumption that the Moon is not rotated by a power or particular force situate within her, but by energy existent in our planet, is invalid and delusory. She is not dependent on the energy of the Earth, for such movement, as a child is dependent for motion on the energy of the mother who carries it. To elucidate.

If it may be that the Moon originally gyrated with greater speed than she now does, which is probable, then the rotation was manifestly an intrinsic and independent movement; and in this case, of course, the present motion must be a movement *per se*, for it is a *remaining portion* of the other.

But if it may be that the Moon originally gyrated with lesser speed than she now does, or hardly at all, then it will not be so manifest how such rotation was accelerated by energy subsistent within her. A critical examination into causes, however, will show it was thus increased. When the Moon assumed her present position in relation to the Earth, the exact amount of potential energy to be transformed into kinetic energy of any kind whatsoever was determined; and since then it was impossible for the available kinetic energy to augment, or to diminish

except through radiation and resistance without a change of relation between the planet and satellite. Whence, then, came the gyratory movement? Since potential energy could not be transformed into kinetic in its production, it was necessarily the effect of kinetic energy until then operating in a different manner. Now, at the expense of what other actual energy was this rotary movement instituted or accelerated? At the expense of the Moon's heat.

Gravity tended to contract the molten mass of the Moon, while heat tended to expand it. In those portions which were in relation with the Earth the influence of the latter neutralized the Moon's own force of gravity to some degree, and the tendency to expand exceeded the tendency to contract. So tides were caused. Perhaps these tides were disturbed, and either hindered or assisted by convection currents.

Another illustration of the cause of tides will not be amiss. Take the Earth. In those portions which are in relation with Sun and Moon, the influences of the latter partially neutralize its own force of gravity. Centrifugal force preponderates, and the impetus of matter on the Earth's surface carries it farther from the Earth's center than at other points. This impetus is the true cause of our tides—not the attraction of the Moon, and they are raised at its expense.

Gravity, in instances such as these, simply presents the occasion on which a cause becomes operative; or, to use the words of the metaphysicians, gravity merely gives causality to causes. In the one case the cause was heat, and in the other case it is the *vis viva* of portions of the Earth in diurnal movement.

Now, I think, it is established that the Moon has a movement of rotation properly so-called, a gyral motion caused by inherent and unborrowed energy. So careful an inquiry has been made into the causes of tides, for the reason that we are apt to err in considering attraction the true cause of them and of the Moon's rotation through them, in which case her rotation would be entirely dependent upon the Earth and would not be a movement *per se*.

However, when we regard the problem in a more particular manner, taking into account the exact time of rotation and revolution of the Moon, a qualification must be made. In brief: the Moon's rotation is dependent upon the Earth in respect of form or period, and to this extent it is not a self-sufficient movement of rotation; but her axial motion is a rotation properly so-called in respect of matter, except that terrestrial attraction

gave causality to the cause thereof.

Uppermost in the minds of the defenders of the thesis is the question whether or not the Moon rotates, while uppermost in the minds of their opponents is the question *how* the Moon rotates, although the latter have confounded this question with the other. Both parties should agree in the conclusion that the manner of rotation is dependent upon the Earth, but that the motion itself is a movement of rotation properly so-called.

Buffalo, New York.

PLANET ORBITS III.

THOMAS CURRAN RYAN

For Popular Astronomy.

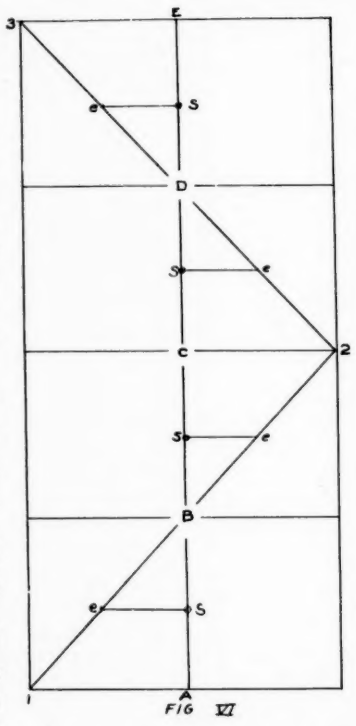
Obliquity of the Spiral. The Element of Position.

It will be kept in mind that this series of articles deals with the orbits as they would appear if observed from space outside of the solar system. In other words, we are studying the helicoidal or spiral paths in which they move around the Sun's way toward the solar apex. We are limiting ourselves to the Earth's spiral, assuming that it is a fair sample of the others except in the relative proportions of length and diameter, shown in my first article; and, as there also shown, those differences in dimension do not imply anything more than that they are necessary in each case in order to render a spiral orbit possible under the individual conditions of velocity mass and distance belonging to each planet. The effects of the contending forces, motion and attraction as manifested in the Earth's spiral, are not prevented in the other orbits by these variations in dimension; they are, on the contrary, thus accomodated, or rendered possible.

I reproduce here Figures VI and VII of the second article, but upon a larger scale in order to admit of some additional lines and indicative letters and numerals in Figure VII, relating to other matters here to be considered.

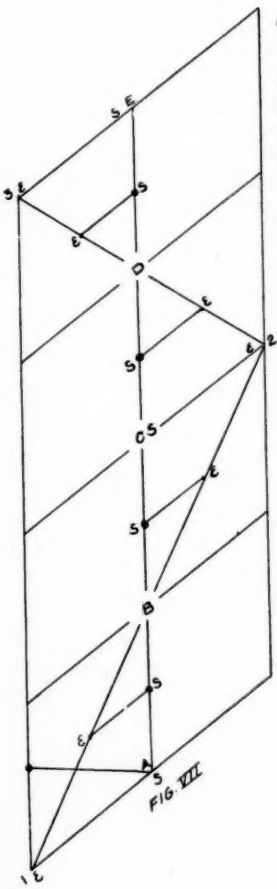
Figure VII, it will be recalled, presents the conditions that would exist if the solar apex inclines away from the ecliptic pole to the extent found by Ludwig Struve, *i. e.*, forty degrees. It also assumes that the Sun's path and the equinoxes on either side thereof lie in one plane; but this would be the case no matter what *direction* the Sun's apex is from the ecliptic pole;

therefore Figure VII will fit any case of a solar apex forty degrees from that pole. The side lines of this figure signify surface elements of the cylinder of space around and upon which the helicoidal path of the Earth would have to run under the circumstances presented, assuming that a relative orbit in the plane of the ecliptic would be practicable in that case. The upper and lower lines of the figure are bases of a section of the cylinder long enough to contain one year's journey of Sun and Earth. These lines, and all imaginary intervening lines parallel to them, are in the ecliptic plane which is constantly shifting toward the solar apex with the motion of Earth and Sun. The line A-B-C-D-E marks the Sun's way. Line 1-B-2 is a chord of the Earth's perihelion arc, and line 2-D-3 a chord of the aphelion arc. These arcs are not shown, but must be imagined. Points 1 and A, and 3 and E, indicate the positions of Earth and Sun at two autumnal equinoxes and points 2 and C their places at the intervening vernal equinox. All these descriptive remarks apply equally to Figure VI, which pictures an ideal condition in which the Sun's way is perpendicular to the ecliptic plane, and the Earth's orbit has no eccentricity.



The two cylinders represented by these figures differ in the respect that cylinder VI is right and cylinder VII is oblique. One effect of obliquity upon the latter is to reduce its diameter; and this makes it impossible that the Earth could be (as in Figure VI), on a line perpendicular to the Sun's path, because if the Earth were so placed in Figure VII its distance from the Sun would be only seventy-two and one-third millions of miles. On account of the obliquity of cylinder VII, and because the Earth's orbit lies in the ecliptic, the constant distance maintained in Figure VI, can become an average distance in Figure VII.

It is the actual distance at points 1-A, 2-C, and 3-E in that figure, as can be demonstrated by measurement. While the Earth remains in the ecliptic these distances are possible at those points in cylinder VII with existing forces, if an orbit is possible, but not at other points with the cylinder's diminished diameter, resulting from its obliquity.



A glance at Figure VII will, however, convince any one that this distance with which the two bodies enter upon their annual journey at the autumnal equinox, 1-A, cannot be maintained. As shown in number II of this series, the velocity and attraction that would cause the Earth to travel the orbit presented in Figure VI would, of necessity, keep it at the same distance from the Sun constantly throughout the year. This would be so because the Earth would constantly be in the same position as related to the Sun's line of motion—that is to say, perpendicular thereto. In other words, they would constantly be "neck and neck" in the race toward the solar apex. But under the conditions shown in Figure VII this is not the case; the Earth at autumnal equinox is not placed *perpendicular* to the Sun's path; on the contrary, as related to that path, or any line running parallel thereto, the Earth is obviously *behind* the Sun. The amount of this handicap and its consequences, are illustrated, for the Earth's perihelion journey, by Figure VII, as follows:—

Point *o* in Figure VII is 58,000,000 miles from point 1; but it is obviously where the Earth would have to be at the beginning of the year in order to be perpendicular to the Sun's path, and give the lines of motion and attraction the same angle relative to each other that they possess in Figure VI, namely 48 degrees; as things are in Figure VII that angle is only 25 degrees. It is readily seen that if the Earth were at *o* when it begins a year's

journey, the chord of its arc would not be parallel to the line 1-B-2, but would run from *o* to a point on the opposite side of the figure as far below 2 as *o* is above 1; also, the aphelion chord would just equal it in length and angular relation to the line of attraction. In other words the conditions would then be what they are in Figure VI, except that the diameter of the cylinder would be much less.

The distance from 1 to A in Figure VII is, by the scale, 93,000,000 miles. That is to say: if the Earth should be at *o* when the Sun is at A, it would be even with the Sun in their apex-ward flight, just as it would always be under conditions

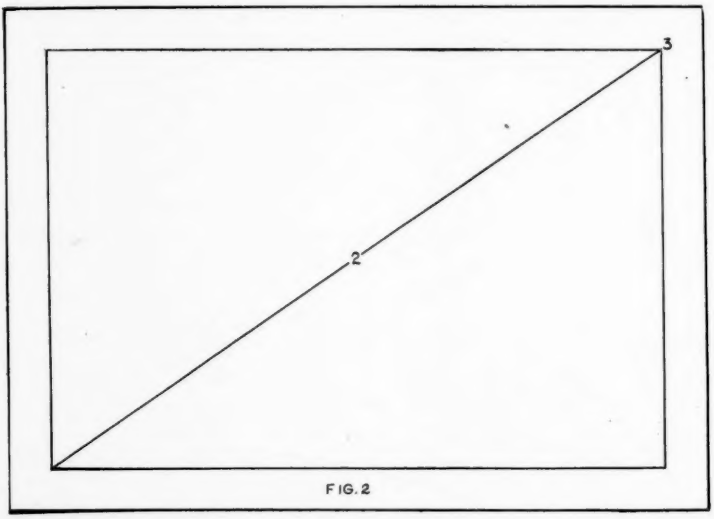


FIG. 2

shown in Figure VI, but if the Earth be then at 1, as it must be in order to make the distance right, it is 58,000,000 miles *behind* the Sun. Obviously, when the Earth is behind the Sun, since both are being translated toward the same apex, (wherever that apex may be) the lines of attraction and of the Earth's apex-ward motion tend in the same direction, and, to the extent of this similarity of tendency, attraction is aided by the Earth's motion. This combination of causes forces three results,—the the Earth's speed is increased, the curvature of its motion grows less, and its distance from the Sun is diminished. These results continue with positive acceleration until the Sun arrives at B and the Earth at the perihelion of its arc,—half-way between 1 and 2. At this half-way point the Earth overtakes the Sun;

then it immediately forges ahead of him, and, though the velocity remains above the average, the acceleration becomes negative until the Earth is at 2 and the Sun at C, where both speed and distance return to the average, although the Earth, as the figure shows, is still ahead of the Sun.

It is also obvious from a glance at Figure VII, that after the Sun passes point B, and the Earth enters on the last half of its perihelion arc, the pull of attraction is in a direction *contrary* to what it was during the preceding quarter year. From A to B the Sun attracts the Earth *toward* the solar apex, and the Earth's motion tends toward that apex also; but after passing B the relative positions of the two bodies are so changed that while the Earth's motion is still toward the solar apex the Sun's attraction is pulling it *away from* that point. Figure VII exhibits a few of these lines of attraction,—the lines S-e (Sun and Earth) running from the chord of the Earth's arc to the Sun's path. Although these lines run from *the chord* of the arc upon which the Earth moves, they nevertheless show accurately the variations occurring in *the angle* between attraction and the Earth's motion because the arc and chord necessarily have the same angular relation to the Sun's path, so that wherever the Earth might be in its arc, its distance *behind, or ahead of, the Sun* would be the same as if the planet were at an equivalent point upon the chord of its arc.

I have been assuming, provisionally, that this orbit outlined in Figure VII, and derived from Ludwig Struve's apex result, is the one in which the Earth actually moves. This involves a further assumption that the velocity of the Earth and the attraction between it and the Sun *could* keep the planet in an orbit of the shape and dimensions imposed by the conditions of Figure VII. In other words, before assuming that this *is* the Earth's orbit we must assume that our planet could travel the arc subtended by the chord 1-B-2, without reducing its least distance from the Sun, below the actual perihelion distance and without travelling a greater number of miles than it actually does. Let us now ascertain, if we can, whether or not this assumption is warranted.

We will find it easier to understand what would be the situation under the conditions of Figure VII if we realize how those shown in Figure VI would result. I have attempted to show that with the forces of motion and attraction unchanged from what they actually are, and with the 48° angle between these forces given in Figure VI, the Earth would move in a perfect, uneccentric orbit, always with the same speed, always at the

same distance from the Sun. So moving, our planet would travel 351,500,000 miles in passing over the arc subtended by chord 1-B-2 in Figure VI; its constant distance from the Sun, meanwhile, would be the distance from 1 to A in both Figures VI and VII, which according to the scale of the figures is 93,000,000 miles. Whether this distance is exactly right or not makes no difference to us here, the essential thing being to make it alike in both cases.

As before shown, we cannot expect the Earth to remain constantly at this distance with which it begins the perihelion journey in Figure VII. And we know that the distance must not be diminished below what would be the actual least distance, which we may call 91,500,000 miles. It is probably a trifle less than that, but the *exact* least distance has not been ascertained, and a close approximation will answer my purpose fully. Let us see, now, what the Earth must do in accomplishing the perihelion arc of its orbit under conditions shown in cylinder VII. We are at once confronted with an absolute impossibility. The same forces which would carry the Earth over its arc 1-B-2 in Figure VI, are required to carry it over the arc 1-B-2 in Figure VII. But *the chord* of the latter arc is 2,500,000 miles longer than the *arc* is in Figure VI. That is to say: if the Earth travelled the *straight line* 1-B-2 in Figure VII it would accomplish 2,500,000 miles more of a journey than it would in negotiating its semi-annual arc in Figure VI. This is in effect to require that twice two, which in cylinder VI produces four, shall in cylinder VII produce between five and six, for the planet must not be permitted to make a straight short cut in any case; it must have an arc which shall not approach the Sun nearer than 91,500,000 miles; it must follow that arc; and such an arc, subtending the chord 1-B-2 in Figure VII, would have a total length of 449,000,000 miles. Thus it is apparent that the Earth would be required to travel 449,000,000 miles in the 15,476,400 seconds of time allowed for perihelion journey, while under the conditions of Figure VI, with exactly the same velocity and attraction, it is able to make only 351,500,000 miles in a full half year—15,778,463 seconds. This is obviously impossible. And, of course, it is inevitable that the difficulties will multiply if we persist in trying to make this orbit fit the case by attempting to preserve the actual least distance.

The chord 1-B-2, which in Figure VI is 274,000,000 miles, is lengthened in Figure VII to 356,000,000 miles, an increase of 82,000,000 miles. The arc subtended by this chord is a segment

of a very large ellipse the circumference of which could be easily ascertained, but it is not important. The major axis of that ellipse is parallel to the chord 1-B-2, so that any lengthening of one lengthens the other proportionately. Therefore, as this chord is lengthened, the major and minor axes of the ellipse are affected proportionately. In other words, as the chord 1-B-2 is lengthened the minor axis at right angles thereto is shortened. It is to be noticed that what I speak of here as the major and the minor axes of the ellipse which would complete the arc whose chord is 1-B-2, are not the equivalents of those axes in the Earth's relative orbit. Also, the ellipse that would complete that arc never does so, never can in a spiral orbit.

When a true circle is compressed through all grades of ellipticity until its lines all vanish in the major axis the *average* loss in length of the minor axis is one mile for each fifty-seven one-hundredths of a mile gained by the major axis. But this average is not maintained throughout the process, and the progressive ratio of gain and loss is a very complex problem involving a branch of the higher analysis—elliptic functions—with which I am not conversant. I do not know whether it has been solved or not. I have seen no solution of it. But the quantity of gain to the major axis shown in Figure VII, over the normal represented by Figure VI, is so large, and involves necessarily so great a loss to the minor axis, that even if we should apply the incredible ratio of mile for mile during these *first* stages of the process, when the ratio is least, it would take away from the radius extending into Figure VII, one half of the 82,000,000 miles added to the major axis, and so would bring the Earth within 51,000,000 miles of the Sun at perihelion. Quite obviously the increased attraction and consequent increase in speed before reaching perihelion would make the distance much less than this.

Evidently, then, Figure VII presents a case of too much obliquity, and, as a result of that, too much eccentricity for the Earth's relative orbit. And quite as evidently there must be some obliquity, since there is some eccentricity, unless eccentricity can be introduced into the relative orbit by some other cause or condition, without removing the Earth from the ecliptic plane.

That there should be more or less obliquity in the cylinder is as probable as that there should be eccentricity in the relative orbit, and for the same reason:—namely, because there can be but one perfectly right cylinder, as there can be but one perfect circle, while obliquity and eccentricity may exist in innumerable

degrees. Obliquity should therefore be always expected; it is clearly the normal, and probably the universal condition. Hence the element of *position* above referred to, must always exist. And this brings into the field of study a new, and important factor to be considered as entering into and affecting strongly the forces from which a planet's orbit is evolved,—namely the effect of *position* of the planet and Sun as related to their apex-ward progress. For if the planet be at any time *behind* the Sun an increased percentage of its motion coincides with attraction. But when the planet is *ahead* of the Sun, these two forces which in the former case were exerted more or less coincidently, are now, on the contrary, expended in directions more or less opposite to each other.

This element of *position* is not accounted for in the relative orbit nor can it be; because the effects manifested in the relative orbit occur in a plane crossing that in which these effects of *position* occur. The relative orbit shows *distance*, and *lateral swing*, and while the term "position" may also be applied to these things, neither of them includes the planet's and Sun's relative positions as to precedence or sequence, in their apex-ward flight, which is all that I mean by the term as used here. With this definition, the word will serve my purpose, though, very likely, a better term might be found, or invented,—one limited strictly to positions of precedence and sequence, *apex-ward*.

Wausau, Wisconsin.

(To be concluded.)

THE WISPS OF SATURN.*

PERCIVAL LOWELL

On September 19, 1909, in examining Saturn I noticed what seemed to be faint lacings traversing diagonally the planet's equatorial belt. Not only was the phenomenon unprecedented, but it was so faint and illusive that I was unable at first to assure myself of its objective reality. On mentioning my impressions to my assistant, Mr. E. C. Slipher, I found to my surprise that he had had suspicion of the same thing on September 9, and had even thought to detect trace of it on the photographs of Saturn taken by him afterwards on that day. At the same

* Lowell Observatory, Bulletin No. 44.

time that I divined rather than descried the wisps I saw unmistakably a fine dark irregular line running longitudinally through the middle of the bright equatorial belt, a line not of uniform character, but as if composed of beads strung up on a wire.

Following up the eye-hint thus vouchsafed me, I examined the equatorial belt for the wisps at moments of good definition during the succeeding days. Repeated observation of them both by me and my assistant eventually showed them to be facts, and then, to clinch the matter, I succeeded in getting, on November 4, some excellent photographs of the planet in which they stood recorded, appearing *in situ* in successive images on the same plate.

The manner in which the photographs are taken enables one to eliminate on the images imperfections due the apparatus from features due the planet itself; for not only is the plate moved first horizontally and then vertically for the successive images, but the color-screen through which the photographing is due is itself shifted at intervals during the taking of the plate. Nothing due to either cause could therefore perpetuate itself *in situ* on images of the set some distance apart.

On the photographic plate the images appear exactly as they do to the eye, though of course not so well defined, to wit: as wisps or filaments, fainter than the dark belts of the planet's disk, crossing the bright equatorial belt. Like all astronomic phenomena previously unperceived, they became much easier to catch after their presence was known; their recognition not being nearly so difficult as their identification. Their apparent lack of contrast with their surroundings is for much in this; which is also why the photographs which intensify contrast brought them out.

My record-book notes on the subject of the lacings and the mid-line in the equatorial belt read as follows:

Sept. 19, 12h. 50m.	Wisps across equatorial belt (drawing of them).
Sept. 23, 11h. 20m.	A longitudinal streak in mid-equatorial belt like that on Jupiter.
	Wisp adumbrated.
40m.	Wisps better seen.
Sept. 27, 11h.	Medial line of belt very distinct—24-inch aperture.
	Wisps insistent, 25mm—no screen, 24-inch aperture. Definition excellent at times, seeing 6 occasionally.
Oct. 2.	Wisps visible; medial line of equatorial belt just perceptible.
Oct. 11, 11h. 54m.	Wisps insistent.
Nov. 4, 10h. 15m.	Wisps easy.
Dec. 8, 9h. 50m.	Wisps tenuous and well defined.

In look the lacings resemble faint shreds of some dusky substance in the act of dropping from points on one edge of the bright belt over to the other. Though darker than the bright belt, they are much lighter than the dark belts from which they come, a difference in tone which is probably due to their real slenderness being spread out by the light-waves and so thinned in strength.

Careful scrutiny revealed that the wisps started from triangular spots in the dark belt, and Mr. G. R. Agassiz, who was observing with me, detected that the adjacent parts of this belt were themselves crisscrossed by darker lines. The triangular spots are not difficult, giving the edge of the belt in good seeing a notched or toothed appearance. It has not been possible as yet to use the wisps for timing the rotation periods of Saturn's equatorial region, owing chiefly to their number and the confusion consequent upon it, but identification will undoubtedly come in time and give us a more accurate value of the equatorial speed than we at present possess.*

Interesting as the lacings are in themselves, they become doubly so from this fact, that they almost precisely parallel the phenomena recently discovered on Jupiter, in the same regions, except for faintness, which Saturn's greater distance both from the Sun and from us alone would cause, the one set of features is almost a replica of the other. Like the Jovian, the wisps seem to be more tenuous in the middle of their course. Like the Jovian equatorial phenomena, too, the medial core is more conspicuous than the wisps.

The Saturnian lacings cross the equatorial belt usually at an angle, but one which is less than that of the average of the Jovian ones. This angle should have something to say about the factors concerned in the formation of the two sets respectively; for analogy is best explained by secondary difference.

Their existence makes plain that one and the same set of causes has operated on both planets to the production of like detail. Now the two planets have much in common in the way of physical conditions, rotation, mean density, size, etc., Saturn going beyond Jupiter in departure from the Earth's state. The most significant fact perhaps is the relative relation borne by the oblateness η to the ratio ϕ between the centrifugal force, so-called, and gravity at the equator, in the cases of the two

* Since this was written, I have timed the crossing of one on the central meridian, but there are so many and they are so alike as to render the method at present impracticable.

planets and of the Earth respectively. Thus, taking the most modern data as basis, I find from an investigation into the possible relation between η and ϕ the following for the three:

Jupiter $\eta = .72\ \phi$
Saturn $\eta = .66\ \phi$
Earth $\eta = .97\ \phi$

As the possible limits of the relation are:

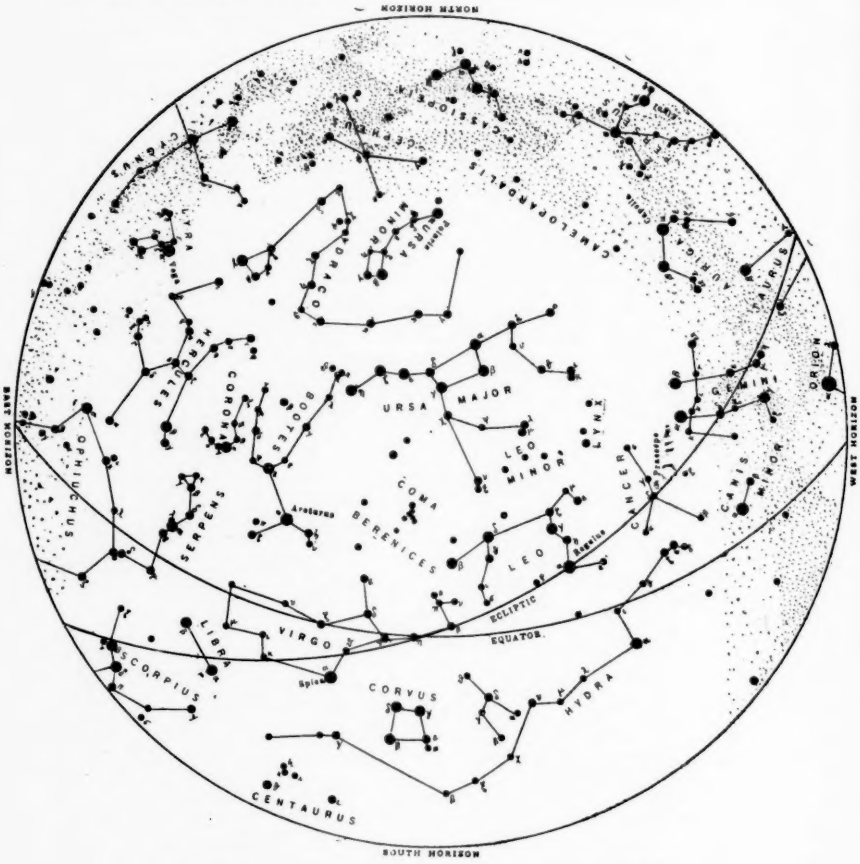
$\eta = .50\ \phi$ for the Earth
$\eta = .487\ \phi$ for Jupiter
$\eta = .476\ \phi$ for Saturn

corresponding to concentration of all the body's substance at its center and $\eta = 1.25\ \phi$ corresponding to homogeneity, for all we see that Jupiter and Saturn come close to concordance in the distribution of their matter while differing entirely from the Earth. Thus their surface matter must be even less dense as compared with mean density than is the case with the Earth, where the one is one-half the other. As their mean densities are but 1.32 and .67 times that of water respectively, their surface matter must be rather gaseous than liquid, gases much condensed in which float their clouds. Saturn is thus indicated to be a vast shell of gaseous matter surrounding a much smaller kernel of denser substance. The existence, then, of lacings or regular persistent rents in these clouds so nearly resembling one another on the two planets, shows that such features are the inevitable outcome of the forces at work under the physical conditions, and give us some clue to the meteorologic processes exemplified there.

February 4, 1910.

PLANET NOTES FOR MAY, 1910.

Mercury will be at greatest elongation, east from the Sun 20° 55′, on May 2 at 10 A. M. central time, and so will be visible in the western sky soon after sunset on several days before and after that time. It will be brighter before than it will be after the time of greatest elongation.



THE CONSTELLATIONS AT 9:00 P. M., MAY 1, 1910.

Venus is brilliant toward the east in the morning. The phase of Venus May 1 is about that of the half Moon, and will become increasingly gibbous during the month, two-thirds of the east and west diameter being illuminated on May 31.

Mars is moving eastward through Gemini during this month and crosses the meridian in the afternoon at about three o'clock. It may be seen low in the west at nine o'clock in the evening.

Jupiter is in good position for observation in the early evening. This planet is in the constellation Virgo just west of the double star Gamma. It is moving slowly westward and will become stationary June 1. The belts of Jupiter are easily seen and with large telescopes much detail can be seen when the definition is good.

Saturn is morning star, too near the Sun for good observation.

Uranus may be observed in the morning in the constellation Sagittarius, but is at too low an altitude for satisfactory study.

Neptune is in excellent position for study. It is in the constellation Gemini not far from the star Delta, but cannot be seen without the aid of a good telescope.

Occultations visible at Washington.

Date 1910	Star's Name	Magnitude.	IMMERSION. Washington M.T. h m	IMMERSION. Angle f'm N. °	EMERSION. Washington M.T. h m	EMERSION. Angle f'm N °	Duration. h m
May 2	69 Aquarii	5.6	16 32	21	17 24	289	0 52
19	46 Virginis	6.1	10 33	167	11 39	270	1 06
19	48 Virginis	6.5	12 43	105	13 52	316	1 09
22	26 Librae	6.3	10 15	83	11 24	342	1 09
23	10 G. Scorpii	5.9	6 50	92	7 49	323	0 59
24	88 B. Ophiuchi	6.3	14 29	93	15 54	285	1 25
24	26 Ophiuchi	5.8	14 41	66	15 52	312	1 11

Phenomena of Jupiter's Satellites.

Central Standard Time.

1910	h	m			1910	h	m		
May 1	12	16	I	Oc. Dis.	May 12	10	50	I	Tr. Eg.
2	7	55	III	Sh. Eg.		12	39	II	Sh. Eg.
	9	34	I	Tr. In.	14	7	41	II	Ec. Re.
	10	18	I	Sh. In.	16	9	28	III	Tr. In.
	11	48	I	Tr. Eg.		11	59	III	Tr. Eg.
	11	48	I	Sh. Eg.		13	09	I	Tr. In.
	12	33	I	Sh. Eg.	17	10	17	I	Oc. Dis.
3	6	42	I	Oc. Dis.	18	7	36	I	Tr. In
	9	40	I	Ec. Re.		8	36	I	Sh. In.
	11	35	II	Oc. Dis.		9	51	I	Tr. Eg.
4	7	01	I	Sh. Eg.		10	51	I	Sh. Eg.
5	7	22	II	Sh. In.	19	7	57	I	Ec. Re.
	8	30	II	Tr. Eg.		10	31	II	Tr. In.
	10	04	II	Sh. Eg.		12	32	II	Sh. In.
9	8	25	III	Tr. Eg.		13	13	II	Tr. Eg.
	9	27	III	Sh. In.	21	10	18	II	Ec. Re.
	11	21	I	Tr. In.	24	12	06	I	Oc. Dis.
	11	53	III	Sh. Eg.	25	9	26	I	Tr. In.
	12	13	I	Sh. In.		10	31	I	Sh. In.
	13	35	I	Tr. Eg.		11	41	I	Tr. Eg.
	14	27	I	Sh. Eg.		12	46	I	Sh. Eg.
10	8	29	I	Oc. Dis.	26	9	52	I	Ec. Re.
	11	34	I	Ec. Re.		12	56	II	Tr. In.
11	6	41	I	Sh. In.	27	7	14	I	Sh. Eg.
	8	03	I	Tr. Eg.		7	30	III	Ec. Dis.
	8	56	I	Sh. Eg.		9	40	III	Ec. Re.
12	8	09	II	Tr. In.	28	7	59	II	Oc. Dis.
	9	57	II	Sh. In.	30	7	7	II	Sh. Eg.

Note.—In., denotes ingress; Eg., egress; Dis., disappearance; Re., reappearance; Ec., eclipse; Oc., occultation; Tr., Transit of the Satellite; Sh., transit of the shadow.

COMET NOTES.

Daniel's Comet *e* 1909.—Daniel's comet is exceedingly faint now. It was observed by Rambaud at Algiers on March 3 at $11^h\ 39^m.4$ Algiers m. t. in $\alpha\ 7^h\ 25^m\ 3^s.40$, $\delta\ +54°\ 54'\ 42''$.

Elements and Ephemeris of Comet *a* 1910.—Approximate elements from my previous computations gave the geocentric distances of the comet for the observations at Rome January 18, and at Washington February 4. The ratio (M) of these distances was varied for three sets of parabolic elements; and the resulting residuals in λ and β, for an observation at Washington, January 25. Log M was then varied in direct proportion to $\Delta\lambda$ and $\Delta\beta$; this gave the following elements.

ELEMENTS.

$T = 1910$ January 17.08840 Gr. M. T.
$\omega = 320°\ 53'\ 47''$
$\Omega = 88\ 46\ 44$ } Ecliptic 1910.0
$i = 138\ 46\ 36$
$\log q = 9.11050$
$(o-c)\ \cos\beta\Delta\lambda = -3''$
$\Delta\beta = -9''$.

CONSTANTS TO THE EQUATOR.

$x = r\,[9.87637]\ \sin(v + 322°\ 31'\ 11'')$
$y = r\,[9.98141]\ \sin(v + 67\ 41\ 56\ )$
$z = r\,[9.85637]\ \sin(v + 354\ 31\ 34\ )$

EPHEMERIS FOR GREENWICH MEAN TIME.

	App. α h	m	s	App. δ °	′	″	log r	log Δ
1910 Mar. 25.0	22	32	7.6	+18	2	0	0.22888	0.39482
29.0	22	34	9.2	18	48	14		
Apr. 2.0	22	35	57.3	19	34	9		
6.0	22	37	31.4	20	19	44		
10.0	22	38	51.0	21	5	1	0.29470	0.42140
14.0	22	39	55.4	21	50	1		
18.0	22	40	43.9	22	34	46		
22.0	22	41	16.0	23	19	15		
26.0	22	41	31.1	+24	3	31	0.34858	0.43574

Correction to the ephemeris from the Washington observation, March 13.9, $\Delta\alpha = -0^s.4$, $\Delta\delta = -29''$ and from the Lick observation, March 13.0, $\Delta\alpha = -0^s.2$, $\Delta\delta = -13''$. HERBERT R. MORGAN.

[Communicated by Capt. T. E. De Witt Veeder, U. S. N., Superintendent, U.S. Naval Observatory.]

Places of Comet *a* 1910.—Found by setting off R. A. and Decl. from Venus or α Pegasi: No correction for refraction. If desired, the refraction correction could be ascertained by direct test.

Gr. M. T.	h	m	R.A. h	m	Decl. °	Remark
Feb. 7	13	47	21	50	+6.52	
8	13	52	21	51.4	+7.00	Very faint trace of tail
11	14	07	21	56.2	+8.3	

University of Arizona, Tucson, Arizona. A. E. DOUGLASS.

The Great Comet *a* 1910.—I observed the great comet here on Jan. 22 and 29, observations on other dates being impossible owing to the unfavorable weather. On the 29th I observed the comet with a two-inch refractor and had an excellent view. The nucleus was very bright and well-defined. The head reminded me of drawings of Coggia's comet of 1874.

Balerno, Midlothian, Scotland. HECTOR MACPHERSON, Jr.

Comet *a* 1910 Observed in Turkey.—Dr. A. G. Sivaslian of Marsovan, Turkey, writes under date Jan. 27 that the comet was seen at Marsovan for the first time on Jan. 24 at 6 P. M., by one of his students in astronomy. At the time of writing Dr. Sivaslian did not know of the discovery of the comet by any one else. He says "I made the enclosed sketch the next day at 6:15 in the evening twilight. I could trace the tail to 10-12 degrees from the head. The full Moon also was above the eastern horizon. I think it would appear much brighter if there was no Moon. If I remember right this is the brightest comet since the great comet of 1882. Daniel's comet in the summer of 1907 was not as bright, though we could trace its tail to 10-12 degrees. I suppose this comet was discovered several weeks earlier, but we are situated so far away from civilized centers of the world that it takes not less than three weeks for us to hear of any new discovery. I have January numbers of POPULAR ASTRONOMY and *The Observatory* in which there is no indication of this comet, so we do not know who is the first discoverer. For us Zachariades (a student in my class in my class in astronomy) is the discoverer, until we hear of the one who anticipated him."

Comet *a* 1910.—A telegram has been received at this observatory from Professor W. W. Campbell, director of the Lick Observatory, stating that Comet *a* 1910 was observed by Astronomer R. G. Aitken March $13^h.0440$ Greenwich mean time, in

R. A. $22^h\ 24\ 43^s.1$ Decl. $+15°\ 38'\ 44''$.

The comet was visible in a small telescope.

A telegram has also been received at this observatory from Capt. T. E. De Witt Veeder, Supt. U. S. Naval Observatory, stating that Comet *a* 1910 was observed by Eppes, at Washington, March $13^h.9256$ Greenwich mean time, in

R. A. $22^h\ 25^m\ 19^s.9$ Decl. $+15°\ 49'\ 17''$.

Notes:—Both of these observations indicate a correction of about $+2^m$ in right ascension and $+0'.4$ in declination to the ephemeris of Kobold given in *Astron. Nach.* **183**, 309.

Astronomical Bulletin, No. 396,
Harvard College Observatory,
Cambridge, Mass., Mar. 15, 1910.

The Discovery of the Great Comet *a* 1910.—The following statement concerning the discovery of Comet *a* 1910 is given in the *Astronomische Nachrichten* 4387, and is an extract from a letter written January 17, by Mr. R. T. A. Innes, director the Transvaal Observatory: "On the 15th inst. the 'Leader' newspaper of Johannesburg informed me by telephone that they had received a telegram from the railway station master at Kopjes (Orange Free State) as follows:

> "Halley's comet was seen by Foreman Bourke, Driver Tricker and Guard Marais at $4^h\ 45^m$ rising in front of Sun. It was visible for about twenty minutes."

Warned by this we kept watch on the next morning (Sunday) but it was cloudy. This morning was also very cloudy, but there was a break just above the place of sunrise. At $5^h\ 29^m$ standard time, the comet was seen independently by Mr. W. M. Worssell and myself but by the former a few seconds earlier.

At the earliest opportunity we informed the South African observatories by telegraph and sent you the cablegram announcing the discovery. Later we secured circle positions with the 9-inch refractor checked by observing Altair and obtained

	1910 Jan. 16	
$21^h\ 29^m.2$ Stand. Time	R.A. $= 19^h\ 50^m\ 28^s$	Decl. $= -25°\ 9'.4$
22 23 .0	$= 19\ 51\ 06$	$= -25\ 3.6$

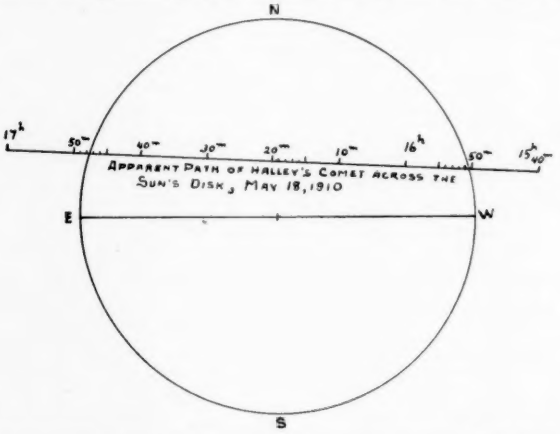
upon which we sent the second cablegram.

If the railway officials at Kopjes were the first to see this comet (which they thought was Halley's), will you please put the fact on record."

A later communication from Mr. Innes to the *Nachrichten* (No. 4389) gives this statement:

"The earliest date on which this comet was seen in South Africa appears to be on Thursday January 12 at $14^h\ 25^m$ Gr. m. t. by workmen at the Transvaal Premier Diamond Mine. A letter from Cullinan, dated Jan. 16 informed me that on that date and also on Friday morning several workmen observed the comet. They described it as an ordinary star with a tail and a little to the right of the point where the Sun rises."

The Transit of Halley's Comet May 18.—Finding the ephemeris which was given in the February number of POPULAR ASTRONOMY, page 116 to be quite exact, representing an observation on Feb. 26 within 8″, we have felt it safe to continue the computation with the same elements through April and May. The conjunction of the comet with the Sun's center in right ascension will occur on May 18 at about $16^h\ 19^m$ Greenwich mean time, the comet being then between 4′ and 5′ north of the center of the Sun's disk. The accompanying diagram shows the apparent path of the comet across the Sun's disk. The transit



APPARENT PATH OF HALLEY'S COMET ACROSS THE SUN'S DISK, MAY 18, 1910.

will occupy less than an hour, beginning at about $15^h\ 50^m$ and ending at about $16^h\ 48^m$ Greenwich mean time. This is after the Sun has set on all parts of the United States. Observers on the islands of the Pacific Ocean will have the rare opportunity of studying the appearance of the head of a great comet as projected upon the face of the Sun, and seen from a distance of only fifteen millions of miles. If there are any solid or semi-solid masses as large as ten miles in diameter in the head of the comet they ought to show as black dots against the Sun. According to Profsssor Barnard's measurements, given in our last number, page 182, the entire head of the comet ought to completely cover the Sun but probably most of it is so tenuous and transparent that it will not materially hinder the solar light.

We in the United States will have the rare privilege of seeing a comet's tail sweep close by us, if we do not actually pass through a portion of it. The Earth will have passed the node of the comet's orbit about an hour before the time of conjunction, so that we will not pass through the axis of the straighter conoids of the comet's tail, but will only graze the outer parts. The broader bent conoids will be so far behind the radius prolonged from the Sun through the comet's nucleus that we shall probably miss them altogether but shall, if the weather be clear, see them sweep slowly across the northern sky. Whether they will stretch entirely across the heavens remains to be seen. On the morning of May 18 the tail of the comet should extend from the eastern horizon toward the west. On the evening of May 19 it should extend from the western horizon toward the east. On this night too the theoretical brightness of the comet reaches its maximum, nearly seven thousand times that at the time of its discovery in September.

Ephemeris of Halley's Comet.

Computed by F. E. Seagrave.

Greenwich Midnight

1910	α			δ			log *r*	log Δ
	h	m	s	°	′	″		
June 1	9	55	59	+2	1	16	0.02430	9.71902
5	10	11	5	+0	37	59	0.04944	9.83076
9	10	29	47	−0	14	16	0.07266	9.91822
13	10	27	52	0	53	15	0.09482	9.99057
17	10	33	27	1	24	27	0.11584	0.05180
21	10	38	8	1	51	5	0.13582	0.10466
25	10	42	14	2	14	46	0.15470	0.15082
29	10	45	57	2	36	47	0.17276	0.19189
July 3	10	49	24	2	57	48	0.18992	0·22859
7	10	52	41	3	18	12	0.20628	0.26167
11	10	55	50	3	38	13	0.22184	0.29159
15	10	58	53	3	58	16	0.23688	0.31901
19	11	1	51	4	18	18	0.25132	0.34412
23	11	4	46	4	38	22	0.26504	0.36706
27	11	7	39	4	58	32	0.27818	0.38814
31	11	10	27	−5	19	2	0.29104	0.40777

New Comet?—The object photographed by Pidoux of Geneva Feb. 20 and announced as a comet does not appear to have been caught again, and must probably be attributed to a photographic defect upon the plate.

Ephemeris of Halley's Comet.

[Continued from page 117.]

Greenwich Midnight 1910	α 1910.0 h m s	δ 1910.0 ° ′ ″	log *r*	log Δ	Br.
April 1.5	0 8 12.5	+8 03 11			
2.5	7 14.9	02 40			
3.5	6 17.0	02 03			
4.5	5 19.0	01 21	9.8288	0.2095	101.7
5.5	4 20.6	8 00 37			
6.5	3 22.0	7 59 49			
7.5	2 23 5	58 56			
8.5	1 24.9	57 59	9.8037	0.1850	127.8
9.5	0 0 26.9	56 56			
10.5	23 59 28.9	55 49			
11.5	58 32.3	54 36			
12.5	57 36.9	53 29	9.7840	0.1549	153.4
13.5	56 43.0	52 19			
14.5	55 50.5	51 12			
15.5	55 00.5	50 09			
16.5	54 13.1	49 11	9.7719	0.1179	201.5
17.5	53 28.5	48 22			
18.5	52 47.2	47 41			
19.5	52 10.0	47 12			
20.5	51 37.2	46 55	9.7690	0.0723	252.0
21.5	51 09.2	46 52			
22.5	50 46.5	47 07			
23.5	50 30.4	47 43			
24.5	50 21.5	48 46	9.7758	0.0169	315.2
25.5	50 21.9	50 21			
26.5	50 29.5	52 31			
27.5	50 49.4	55 19			
28.5	51 14.7	7 58 51	9.7912	9.9519	396.0
29 5	51 53.1	8 03 13			
30.5	52 44.7	08 31			
May 1.5	53 54.3	14 56			
2.5	55 15.2	22 36	9.8133	9.8660	531.3
3.5	57 07.2	32 15			
4.5	23 59 06.7	42 35	9.8262	9.8167	628.2
5.5	0 01 50.7	8 55 30			
6.5	4 43.5	9 10 29	9.8399	9.7611	762.2
7.5	8 44.5	9 28 23			
8.5	12 53.2	9 49 34	9.8541	9.6975	956.5
9.5	18 50.0	10 14 42			
10.5	24 52.0	10 44 32	9.8688	9.6240	1254
11.5	42 58.5	11 20 29	9.8762	9.5827	1466
12.5	43 04.5	12 03 45	9.8837	9.5380	1740
13.5	0 55 50.3	12 56 04	9.8912	9.4894	2103
14.5	1 12 14.6	13 59 12	9.8986	9.4369	2587
15.5	1 33 43.0	15 14 50	9.9061	9.3806	3240
16.5	2 02 13.3	16 41 56	9.9135	9.3220	4100
17.5	2 40 14.3	18 13 38	9.9210	9.2644	5166
18.5	3 29 53.4	19 29 03	9.9283	9.2151	6267
18.650	3 38 22.4	19 37 20	9.9294	9.2090	
18.675	3 39 48.7	19 38 28	9.9296	9.2081	
18.700	3 41 14.9	19 39 38	9.9298	9.2071	
19.5	4 30 25.3	19 51 55	9.9357	9.1848	6966
20.5	5 35 17.0	18 51 24	9.9429	9.1835	6776
21.5	6 34 57.6	16 39 40	9.9501	9 2115	5763
22.4	7 23 29.4	14 01 36	9.9573	9.2592	4476
23.5	8 00 41.0	11 32 44	9.9644	9.3158	3340
24.5	8 28 37.8	9 27 02	9.9714	9.3737	2476
25.5	8 49 49.3	7 44 59	9.9783	9.4294	1856
26.5	9 06 09.9	+6 22 55	9.9852	9.4814	1415

Photographs of Halley's Comet.—In the Lick Observatory Bulletin No. 174 Mr. Heber D. Curtis gives an account of photographs of Halley's comet taken with the Crossley reflector and the Crocker photographic telescope. The negatives of December 11, 12 and 13 show the brighter portion of the nucleus and traces of a faint cone-shaped tail. On the negative of January 28 the tail can be traced nearly a degree. A negative of February 4 shows a very fine sharp stellar nucleus less than five seconds of arc in diameter and the axis of the tail appears as a narrow, sharply defined cone with its base at the head. The photographs of February 10 and 11 show an entirely different form of tail due doubtless to a sudden outburst of activity. The narrow quiescent cone of February 4 and 5 has disappeared and given place to a tail made up of several fine streamers radiating from the head which can be traced fully 20′ from the head. On the plate of February 10 the two longest streamers are straight while the most southerly one is slightly curved. Other negatives record the tail for a distance of about 40′ from the head.

Comet Halley.—Since the note in February POPULAR ASTRONOMY, I have seen the comet on the following nights, the notes taken from my note-book:

Jan. 19.—Comet possibly visible under moonlight.

Feb. 1.—Comet much brighter, but considerably diffuse, seems some elongated to northwest, light haze. Possibly this elongation was caused by a faint star, which in the haze was not clearly perceptible.

Feb. 3.—Comet round with sharp central condensation, about ninth magnitude.

Feb. 6.—Comet much brighter, with faint extension from Sun for about 2′.

Feb. 7.—Comet round with nucleus, and faint tail extension for two diameters from head.

Feb. 10.—Comet much brighter, with sharp nucleus, and faint tail extension, which is difficult owing to clouds and haze.

Comet *a* 1910, was seen at its best on January 29, when it showed with a bright head and two or three magnitude nucleus, and tail which was nearly straight, excepting for the outer end which was slightly curved to the south, and could be traced for over 20° from head. There was secondary tail to the south of main tail and beautifully curved away from the other, and separating from main tail at three-fourths of a degree from head.

February 10, 1910. WM. E. SPERRA.

Halley's Comet with a Small Telescope.—The night of Feb. 2nd., 1910, was remarkably clear, preceding rain. At 8 P. M. I began to search for Halley's comet for the first time, with my tripod refractor of 2¾″ clear aperture. Sweeping back and forth I came upon it at 8:25 P. M. about 4° to the right of Saturn. It was then not more than 15° or 20° above the horizon in the west. Its position was ¾° above the 4th magnitude star ϵ in Pisces, and was estimated from Klein's star atlas to be situated $0^h\ 57^m$, N 7° 30′. It appeared, in 90 power, as a very faint whitish glow, like a nebula. It was circular and seemed to be slightly brighter at the center. By means of the field diameter of 22′ and the help of small stars, I estimated the diameter of the comet to be 1′.

I located in a sketch its position with reference to five telescopic stars in the field, and observed it again Feb. 4th at 7:20 P. M. on an exceptionally fine night, after rain and wind. It was then about 35° above the horizon and considerably plainer. It appeared, in 90 power, as a good sized whitish cloud, and

resembled a condensed star cluster. It was circular and somewhat brighter at the center, although the whitish glow was pretty uniform throughout. Its estimated diameter was 1½′. It had moved about 24′ in two days, and was 33′ above and to the right of ϵ in Pisces.

I observed it again Feb. 5th at 7:45 P. M., and found it had moved between 12′ and 14′ during the 24 hours. There was no tail, and one would naturally take it for a small nebula. From its good size and plainess it was evident that it might have been seen with this sized telescope several weeks sooner.

Folsom, Penn., March 4, 1910. E. W. ABELL.

VARIABLE STARS.

New Elements of Algol-Type Variables RV, RY and ST Persei.—In *A. N.* 4386 Mr. A. A. Nijland of Utrecht gives new elements of three variables in the constellation Perseus.

	α 1900	δ 1900	Minimum
RV Persei	$4^h\ 04^m\ 09^s$	$+33°\ 59'.4$	$2418629.519 + 1^d.973425$ E
RY Persei	2 38 58	+47 43 .2	$2417523.480 + 6$ .863614 E
ST Persei	2 53 43	+38 47 .5	$2418548.505 + 2$ .64833 E

Elements of the Algol-Type Variable Y Camelopardalis.—Mr. Nijland also finds that the elements of Y Camelopardalis require correction and that the observed minima are slightly better represented by the use of a term involving the square of the number of periods. The improvement is so slight that we prefer to use the simpler formula

$$\text{Min.} = 2416306\ 391 + 3.305532\ E.$$

Three Algol-Type Variables.—In *A. N.* 4386 Mr. Sigurd Enebo gives preliminary elements for three Algol stars determined from his own observations.

	α 1900	δ 1900	Minimum	
16.1908 Vulpeculae	$19^h\ 13^m\ 25^s$	$+22°\ 15'.7$	2418542.42 Gr. m. t.	+ 4.477 E
51.1908 Geminorum	5 54 33	+24 28 .1	8662.46	+ 4.007 E
43.1909 Draconis	18 03 03	+58 23 .3	8652.42	+ 5.16 E

Elements of Variable 147.1907 Arietis.—In *A. J.* 609 Mr. Paul S. Yendell gives the following provisional elements of the variable star 147.1907 Arietis:

$$\text{Max.} = \text{J. D. } 2418267.121 \text{ Gr. m. t.} + 0^d.99248\ E.$$

The star is DM + 11°305 and its position for 1900 is

$$\alpha = 2^h\ 9^m\ 36.^s8\ ; \quad \delta = +11°\ 46'\ 18''.$$

"The light range is $0^m.76$, from $8^m.28$ to $9^m.04$ being about that usual in variables of this type. The maximum follows the minimum at an interval of 0.348 day. The maximum phase, though flat, is fairly well determined, though the decline is slow at first, falling only about two-tenths of a magnitude in the four-tenths of a day immediately following the maximum; the final decrease to the minimum seems to be pretty rapid."

Mr. Yendell gives as his comparison stars and light-scale :

		Magn.	Lt.
a	DM +11°300	7.6	19.7
e	+11 309	8.0	15.6
b	+11 307	8.3	10.6
c	+12 308	8.9	5.7
d	+11 304	9.5	0.0

Variables 17. and 43.1909 Draconis.—The variable stars 17 and 43.1909 turn out to be the same star. The former was wrongly identified by Wolf as BD +58°1782. Later Miss Whitney found the star BD +58°1785, which is only 6′ distant from the other, to be variable, and it was given the provisional number 43.1909. In *A. N.* 4389 the observers at Uccle call in question the variability of the former star, and Professor Wolf on looking the matter up finds that the identification was wrong. The two discoveries pertain to the same star BD +58°1785 which is now to be designated SX Draconis. Its position for 1900.0 is

$$\alpha = 18^h\ 03^m\ 03^s.50 \qquad \delta = +58°\ 23'\ 18''.7$$

and its range of brightness from $9^m.3$ to $10^m.5$ in a period of 5 days 4 hours and 16 minutes.

New Variable 2.1910 Pegasi.—The variable DM +34°4600 referred to in the February number of POPULAR ASTRONOMY, page 127 has been given the provisional designation 2.1910 Pegasi.

New Variables 3. and 4.1910 Pegasi.—These are announced in *A. N.* 4386 by W. E. Sperra, and were used by him as comparison stars for the variable 162.1907 Pegasi. Both stars range from about 8.5 to 9.0 magnitude. Their positions for 1900 are

3.1910	$\alpha = 21^h\ 59^m\ 47^s.90$	$\delta = +35°\ 16'\ 30''.8$
4.1910	22 1 21 .26	+35 24 47 .1

New Variable 5.1910 Pegasi.—In *A. N.* 4387 Professor W. Ceraski calls attention to a new variable star discovered by Mme. L. Ceraski upon the Moscow photographs. The star is a faint one, its brightness ranging from magnitude 11.2 to 10.4 as follows :

1909 Sept. 11	$11^m.2$	Oct. 15	$10^m.4$
16	10 .6	19	10 .7
21	10 .6		

Its position for 1900 is

$$\alpha = 22^h\ 8^m\ 16^s; \qquad \delta = +17°\ 55'.$$

New Variable in Aquila.—This is announced in *A. J.* 609 by Professor Mary W. Whitney of Vassar College Observatory as discovered by Dr. Caroline Furness. The star is BD −7°4896, about 15ˢ west and 2′ north of W Aquilae. During the period of observation the magnitude fell from 10 to 11. The variability has been confirmed by Professor Pickering from the Harvard plates. The position for 1900 is

$$\alpha = 19^h\ 09^m\ 44^s.5; \qquad \delta = -7°\ 11'.0$$

Minima of Variable Stars of the Algol Type.

[Given to the nearest hour in Greenwich Mean Time beginning with noon. To reduce to Central Standard time subtract 6 hours, or for Eastern time subtract 5 hours, etc. For stars marked with an * alternate minima are given; ** every third minimum; † every tenth minimum.]

SY Androm.

d	h
May 15	19

***U Cephei**

d	h
May 4	17
9	16
14	15
19	15
24	14
29	14

***Z Persei**

d	h
May 2	3
8	6
14	9
20	11
26	14

RY Persei

d	h
May 1	8
8	4
15	1
21	22
28	19

****RZ Cassiop.**

d	h
May 1	8
4	22
8	12
12	22
15	16
19	6
22	20
26	10
30	0

***ST Persei**

d	h
May 5	10
10	17
16	0
21	7
26	14
31	21

RX Cephei

d	h
May 11	19

***Algol**

d	h
May 5	18
11	12
17	5
22	23
28	17

****RT Persei**

d	h
May 2	8
4	21
7	10
9	23

RT Persei

d	h
May 12	12
15	1
17	15
20	4
22	17
25	6
27	19
30	8

λ Tauri

d	h
May 3	1
7	0
10	23
14	21
18	20
22	19
26	18
30	17

***RW Tauri**

d	h
May 1	7
6	20
12	9
17	22
23	11
28	23

***RV Persei**

d	h
May 3	7
7	6
11	5
15	4
19	2
23	1
27	0
30	23

RW Persei

d	h
May 9	19
23	0

RS Cephei

d	h
May 12	18
25	4

***RY Aurigæ**

d	h
May 6	9
11	20
17	7
22	18
28	5

***RZ Aurigæ**

d	h
May 2	10
8	11
14	12
20	12
26	13

***RW Gemin.**

d	h
May 6	10
12	3
17	21
23	14
29	8

***U Columbæ**

d	h
May 6	5
11	19
17	10
23	0
28	15

RW Monoc.

d	h
May 1	19
5	15
9	10
13	6
17	1
20	21
24	16
28	11

RX Geminorum

d	h
May 8	6
20	11

****RU Monoc.**

d	h
May 2	19
4	14
6	9
8	4
9	23
11	18
13	13
15	8
17	3
18	22
20	17
22	12
24	7
26	2
27	21
29	16
31	11

RY Geminorum

d	h
May 4	14
13	21
23	4

****R Canis Maj.**

d	h
May 2	8
5	18
9	3
12	13

R Canis Maj.

d	h
May 15	23
19	9
22	18
26	4
29	14

***Y Camelop.**

d	h
May 6	18
13	8
19	23
26	14

RR Puppis

d	h
May 4	8
10	19
17	5
23	15
30	2

****V Puppis**

d	h
May 2	3
6	12
10	20
15	5
19	14
23	23
28	7

†X Carinæ

d	h
May 3	4
8	14
14	0
19	10
24	20
30	6

S Cancri

d	h
May 1	17
11	4
20	16
30	4

S Velorum

d	h
May 2	22
8	20
14	19
20	17
26	15

****Y Leonis**

d	h
May 2	16
7	18
12	19
17	21
22	22
27	23

****RR Velorum**

d	h
May 5	0
10	13
16	3
21	16
27	6

***SS Carinae**

d	h
May 5	21
12	12
19	2
25	17

RW Urs. Maj.

d	h
May 4	22
12	6
19	14
26	21

****Z Draconis**

d	h
May 3	20
7	22
12	0
16	2
20	3
24	5
28	7

***SS Centauri**

d	h
May 2	20
7	18
12	17
17	16
22	15
27	14

***δ Libræ**

d	h
May 3	19
8	10
13	2
17	18
22	9
27	1
31	17

***U Coronæ**

d	h
May 6	15
13	12
20	10
27	8

***SW Ophiuchi**

d	h
May 5	8
10	6
15	3
20	0
24	22
29	19

Minima of Variable Stars of the Algol Type.—Continued.

*SX Ophiuchi

	d	h
May	1	10
	5	13
	9	16
	13	19
	17	22
	22	1
	26	4
	30	7

R Aræ

	d	h
May	3	12
	7	22
	12	8
	16	18
	21	5
	25	15
	30	1

**U Ophiuchi

	d	h
May	3	19
	6	7
	8	19
	11	8
	13	20
	16	9
	18	21
	21	9
	23	22
	26	10
	28	22
	31	11

**SZ Herculis

	d	h
May	1	0
	3	11
	5	22
	8	9
	10	20
	13	7
	15	18
	18	5
	20	15
	23	2
	25	13
	28	0
	30	11

Z Herculis

	d	h
May	4	0
	8	0
	10	23
	15	23
	19	23
	23	23
	27	23
	31	23

*RS Sagittarii

	d	h
May	2	0
	6	20
	11	16
	16	11
	21	7
	26	3
	30	23

*V Serpentis

	d	h
May	6	17
	13	14
	20	12
	27	10

†RZ Draconis

	d	h
May	4	1
	10	13
	16	1
	21	14
	27	2

**RX Herculis

	d	h
May	3	2
	5	18
	8	10
	11	2
	13	18
	16	10
	19	2
	21	18
	24	10
	27	3
	29	19

*SX Sagittarii

	d	h
May	1	16
	5	20

SX Sagittarii

	d	h
May	9	23
	14	3
	18	7
	22	10
	26	14
	30	18

*RR Draconis

	d	h
May	1	7
	6	22
	12	14
	18	6
	23	22
	29	14

**U Scuti

	d	h
May	3	10
	6	6
	9	3
	12	0
	14	21
	17	17
	20	14
	23	11
	26	8
	29	4

*RX Draconis

	d	h
May	2	3
	5	22
	9	16
	13	11
	17	6
	21	1
	24	20
	28	14

*RV Lyræ

	d	h
May	8	3
	15	8
	22	13
	29	17

*U Sagittae

	d	h
May	5	1
	11	19
	18	14
	25	8

Z Vulpeculæ

	d	h
May	3	8
	8	9
	13	10
	18	11
	23	12
	28	14

SY Cygni

	d	h
May	3	2
	9	2
	15	2
	21	3
	27	3

*WW Cygni

	d	h
May	7	10
	14	1
	20	16
	27	8

ZW Cygni

	d	h
May	5	12
	10	2
	14	15
	19	5
	23	19
	29	9

VW Cygni

	d	h
May	1	11
	9	22
	18	8
	26	18

*UW Cygni

	d	h
May	7	20
	14	17
	21	15
	28	13

W Delphini

	d	h
May	1	22
	6	17
	11	13
	16	8
	21	4
	25	23
	30	18

RR Delphini

	d	h
May	3	3
	7	17
	12	7
	16	22
	21	12
	26	2
	30	17

RR Vulpeculae

	d	h
May	5	19
	10	11
	15	12
	20	13
	25	14
	30	15

**VV Cygni

	d	h
May	2	21
	7	8
	11	18
	16	4
	20	15
	25	1
	29	11

UZ Cygni

	d	h
May	19	11

*RT Lacertae

	d	h
May	3	12
	8	13
	13	15
	18	17
	23	19
	28	20

*TT Androm.

	d	h
May	4	22
	10	10
	15	23
	21	12
	27	0

21.1909 Andr.

	d	h
May	1	19
	5	21
	10	0
	14	3
	18	6
	22	9
	26	11
	30	14

Maxima of Variable Stars of Short Period not of the Algol Type.

Unless otherwise indicated the times of maxima only are given; and the times of minima may be found by subtracting the interval printed in parentheses under the names of the stars.

SY Cassiop.

	d	h
May	2	22
	6	23
	11	1
	15	3
	19	5
	23	6

SY Cassiop.

	d	h
May	27	8
	31	10

RW Cassiop.

(−5 19)

	d	h
May	2	2
	16	22

RW Cassiop.

	d	h
May	31	17

SU Cassiop.

(−0 22)

	d	h
May	1	21
	3	20

SU Cassiop.

	d	h
May	5	19
	7	17
	9	16
	11	15
	13	14
	15	12

SU Cassiop.

	d	h
May	17	11
	19	10
	21	9
	23	8
	25	6
	27	5

Maxima of Variable Stars of Short Period not of the Algol Type.-Con.

SU Cassiop.

d	h
29	4
May 31	3

SV Persei

May 7	17
18	20
29	23

RX Aurigæ

(−4	0)
May 11	20
23	10

SX Aurigae

May 1	19
3	7
4	21
6	9
7	22
9	10
10	23
12	12
14	1
15	13
17	2
18	15
20	3
21	16
23	5
24	18
26	7
27	19
29	8
30	21

SY Aurigae

May 4	10
14	13
24	17

Y Aurigæ

(−0	18)
May 1	10
5	6
9	3
12	24
16	20
20	17
24	13
28	10

RZ Geminorum

(−1	7)
May 6	9
11	21
17	9
22	22
28	10

RS Orionis

(−2	0)
May 2	23
10	12
18	2
25	15

T Monoc.

d	h
(−9	23)
May 25	19

W Geminorum

(−2	2)
May 7	15
15	13
23	11
31	9

ζ Geminorum

(−5	0)
May 2	3
12	7
22	10

RU Camelop.

(−9	12)
May 8	7
30	14

V Carinæ

(− 2	4)
May 4	3
10	20
17	12
24	5
30	22

T Velorum

(− 1	10)
May 2	11
7	2
11	17
16	9
21	0
25	15
30	7

W Carinæ

(− 1	0)
May 1	17
6	2
10	11
14	20
19	5
23	14
27	23

S Muscæ

(−3	11)
May 3	10
13	2
22	17

T Crucis

(− 2	2)
May 6	8
13	2
19	20
26	13

R Crucis

(−1	10)
May 4	2
9	21
15	17
21	13
27	9

S Crucis

d	h
(− 1	12)
May 5	14
10	7
14	23
19	16
24	8
29	1

RZ Centauri

May 1	13
2	11
3	10
4	8
5	7
6	5
7	4
8	2
9	1
9	23
10	22
11	20
12	19
13	17
14	16
15	14
16	13
17	11
18	10
19	8
20	7
21	5
22	4
23	2
24	1
24	23
25	22
26	20
27	19
28	17
29	16
30	14
31	13

W Virginis

(−8	5)
May 12	22
30	5

V Centauri

(−1	11)
May 2	3
7	15
13	2
18	14
24	8
29	20

R Triang. Austr.

(− 1	0)
May 2	1
5	10
8	20
12	5
15	14

R Triang. Austr.

d	h
May 18	24
22	9
25	18
29	4

S Triang. Austr.

(− 2	2)
May 6	7
12	15
18	23
25	6
31	14

S Normæ

(−4	10)
May 1	22
11	16
21	10
31	4

RV Scorpii

(−1	10)
May 4	17
10	19
16	20
22	22
28	23

RV Ophiuchi Minimum

May 2	22
6	15
10	7
13	24
17	16
21	9
25	1
28	18

X Sagittarii

(−2	22)
May 7	7
14	7
21	8
28	8

Y Ophiuchi

(− 6	5)
May 3	11
20	14

W Sagittarii

(−3	0)
May 6	3
13	17
21	7
28	21

Y Sagittarii

(− 2	2)
May 2	19
8	13
14	8
20	3
25	21
31	16

U Sagittarii

d	h
(− 2	23)
May 1	2
7	20
14	14
21	8
28	2

β Lyræ

(− 3	7)
(− 3	2)
May 4	18
12	0
17	16
24	22
31	14

κ Pavonis

(−4	7)
May 1	8
10	10
19	12
28	14

U Aquilæ

(− 2	4)
May 3	15
10	15
17	16
24	17
31	17

U Vulpeculæ

(− 2	3)
May 4	19
12	18
20	18
28	17

SU Cygni

(− 1	7)
May 2	12
6	8
10	4
14	1
17	21
21	17
25	13
29	10

η Aquilae

(− 2	6)
May 3	18
10	22
18	3
25	7

S Sagittae

(−3	10)
May 8	4
16	13
24	22

X Vulpeculae

(−2	1)
May 5	10
11	17

Maxima of Variable Stars of Short Period not of the Algol Type.

Continued.

X Vulpeculae

	d	h
May	18	1
	24	9
	30	16

X Cygni

	(−6	19)
May	1	7
	17	16

T Vulpeculae

	(−1	10)
May	3	23
	8	10
	12	20
	17	7
	21	17
	26	4
	30	14

TX Cygni

May	3	8
	18	1

VY Cygni

	(−2	14)
May	7	11
	15	8
	23	4
	31	1

VZ Cygni

	(−1	1)
May	1	9
	6	5
	11	2
	15	23
	20	19
	25	16
	30	13

Y Lacertae

	(−1	10)
May	4	14
	8	22
	13	6

Y Lacertae

	d	h
May	17	14
	21	21
	26	5
	30	13

δ Cephei

May	4	5
	9	14
	14	23
	20	8
	25	16
	31	1

Z Lacertae

May	9	20
	20	17
	31	14

RR Lacertae

	(−1	2)
May	7	10
	13	20

RR Lacertae

	d	h
May	20	6
	26	16

V Lacertae

	(−1	16)
May	2	9
	7	8
	12	8
	17	8
	22	7
	27	7

X Lacertae

May	3	2
	13	24
	19	10
	24	21
	30	8

SW Cassiop.

	d	h
May	5	23
	11	10
	16	20
	22	6
	27	17

RS Cassiop.

	(−1	19)
May	1	9
	7	16
	13	23
	20	6
	26	13

RY Cassiop.

	(−7	10)
May	7	11
	19	15
	31	18

Approximate Magnitudes of Variable Stars on Mar. 1, 1910.

[Communicated by the Director of Harvard College Observatory, Cambridge, Mass.]

Name.	R. A. 1900. h	m	Decl. 1900. °	′	Magn.	Name.	R.A. 1900. h	m	Decl. 1900. °	′	Magn.
X Androm.	0	10.8	+46	27	14.5*d*	U Ceti	2	28.9	−13	35	7.8*d*
T Androm.		17.2	+26	26	14.2*d*	RR Cephei		30.4	+80	42	13.3*d*
T Cassiop.		17.8	+55	14	8.3 *i*	R Trianguli		31.0	+33	50	9.8 *i*
R Androm.		18.8	+38	1	10.4*d*	T Arietis		42.8	+17	6	9.3
S Ceti		19.0	− 9	53	11.7*d*	W Persei		43.2	+56	34	8.9
Y Cephei		31.3	+79	48	12 *i*	U Arietis	3	5.5	+14	25	10 2*d*
U Cassiop.		40.8	+47	43	<14.5	X Ceti		14.3	− 1	26	8.5*d*
RW Androm.		41.9	+32	8	13.0	Y Persei		20.9	+43	50	9.0 *i*
V Androm.		44.6	+35	6	13.0 *i*	R Persei		23.7	+35	20	9.0*d*
RR Androm.		45.9	+33	50	13.0 *i*	Nov. Per. 2		24.4	+43	34	12.3*d*
RV Cassiop.		47.1	+46	53	<13.5	T Eridani		51.0	−24	20	11.8*d*
W Cassiop.		49.0	+58	1	8.8	W Eridani	4	7.3	−25	24	<13
Z Ceti	1	1.6	− 2	1	12.0 *i*	T Tauri		16.2	+19	18	11.8 *i*
U Androm.		9.8	+40	11	13.0 *i*	R Tauri		22.8	+ 9	56	9.5 *i*
S Piscium		12.4	+ 8	24	<13 5	W Tauri		22.2	+15	49	9.9 *i*
S Cassiop.		12.3	+72	5	8.3*d*	S Tauri		23.7	+ 9	44	11.0 *i*
U Piscium		17.7	+12	21	<14	T Camelop.		30.4	+65	57	8.0 *i*
R Piscium		25.5	+ 2	22	9.0 *i*	RX Tauri		32.8	+ 8	9	12.5
RU Androm.		32.8	+38	10	12.4*d*	X Camelop.		32.6	+74	56	11.5*d*
Y Androm.		33.7	+38	50	14.7	V Tauri		46.2	+17	22	13.0*d*
X Cassiop.		49.8	+58	46	10.0 *i*	R Orionis		53.6	+ 7	59	10.0
U Persei		53.0	+54	20	8.6	R Leporis		55.0	−14	57	7.6*d*
S Arietis		59.3	+12	3	<14.5	V Orionis	5	0.8	+ 3	58	10.0*d*
R Arietis	2	10.4	+24	35	7.9*d*	T Leporis		0.6	−22	2	12.5*d*
W Androm.		11.2	+43	50	12.8*d*	R Aurigae		9.2	+53	28	9.0 *i*
Z Cephei		12.8	+81	13	11.0	S Aurigae		20.5	+34	4	9.0*d*
o Ceti		14.3	− 3	26	9.0*d*	W Aurigae		20.1	+36	49	<14
S Persei		15.7	+58	8	10.0*d*	S Orionis		24.1	− 4	46	8.3*d*
R Ceti		20.9	− 0	38	9.6*d*	T Orionis		30.9	− 5	32	10.7
RR Persei		21.7	+50	49	13.5	S Camelop.		30.2	+68	45	8.8 *i*

Approximate Magnitudes of Variable Stars on Mar. 1, 1910—Con.

Name.	R. A. 1900. h	m	Decl. 1900. °	′	Magn.	Name.	R. A 1900 h	m	Decl. 1900 °	′	Magn.
RR Tauri	5	33.3	+26	19	11.2*d*	RS Urs. Maj.	12	34.4	+59	2	10.5 *i*
U Aurigae		35.6	+31	59	10.6*d*	S Urs. Maj.		39.6	+61	38	10.5*d*
SU Tauri		43.2	+19	2	<14	RU Virginis		42.2	+ 4	42	11.0
Z Tauri		46.7	+15	46	12.2*d*	U Virginis		46.0	+ 6	6	9.1*d*
U Orionis		49.9	+20	10	11.8	RT Virginis		57.6	+ 5	43	8.7
V Camelop.		49.4	+74	30	<14	RV Virginis	13	2.7	−12	38	10.4
Z Aurigae		53.6	+53	18	9.8	V Virginis		22.6	− 2	39	8.2 *i*
X Aurigae	6	4.4	+50	15	9.2*d*	R Hydrae		24.2	−27	46	7.6*d*
V Aurigae		16.5	+47	45	8.8 *i*	S Virginis		27.8	− 6	41	10.0*d*
V Monoc.		17.7	− 2	9	14.0	R Can. Ven.		44.6	+40	2	10.9*d*
R Monoc.		33.7	+ 8	49	11.3	RR Viriginis		59.6	− 8	43	13.5
S Lyncis		35.9	+58	0	9.6 *i*	Z Bootis	14	1.7	+13	59	<14
X Gemin.		40.7	+30	23	10.5*d*	Z Virginis		5.0	−12	50	12.0
W Monoc.		47.5	− 7	2	11.0*d*	U Urs. Min.		15.1	+67	15	12.0*d*
Y Monoc.		51.3	+11	22	10.0 *i*	S Bootis		19.5	+54	16	13.0
X Monoc.		52.4	− 8	56	8.8	RS Virginis		22.3	+ 5	8	12.5
R Lyncis		53.0	+55	28	9.5*d*	V Bootis		25.7	+39	18	7.8 *i*
RS Gemin.		55.2	+30	40	10.5*d*	R Camelop.		25.1	+84	17	13.3
V Can. Min.	7	1.5	+ 9	2	<14	R Bootis		32.8	+27	10	13.0
R Gemin.		1.3	+22	52	12.5*d*	V Librae		34.8	−17	14	13.5
R Can. Min.		3.2	+10	11	8.1	U Bootis		49.7	+18	6	10.0
RR Monoc.		12.4	+ 1	17	12.3*d*	RT Librae	15	0.8	−18	21	13.8
V Gemin.		17.6	+13	17	11.3 *i*	T Librae		5.0	−19	38	14.0
S Can. Min.		27.3	+ 8	32	11.5 *i*	S Librae		15.6	−20	2	8.5
T Can. Min.		28.4	+11	58	<14	S Serpentis		17.0	+14	40	13.0
Z Puppis		28.3	−20	27	12.0*d*	S Coronae		17.3	+31	44	8.0 *i*
U Can. Min.		35.9	+ 8	37	9.8 *i*	RS Librae		18.5	−22	33	13.0*d*
S Gemin.		37.0	+23	41	11.5*d*	RU Librae		27.7	−14	59	8.0 *i*
T Gemin.		43.3	+23	59	11.4 *i*	X Librae		30.4	−20	50	12.3
U Puppis		56.1	−12	34	<14	W Librae		32.2	−15	51	<14
R Cancri	8	11.0	+12	2	11.4*d*	S Urs. Min.		33.4	+78	58	9.0 *i*
V Cancri		16.0	+17	36	8.9 i	U Librae		36.2	−20	52	9.3
RT Hydrae		24.7	− 5	59	8.0 *i*	— Librae		36.2	−20	28	10.8
U Cancri		30.0	+19	14	12.2*d*	Z Librae		40.7	−20	49	13.5
X Urs. Maj.		33.7	+50	30	<13.5	R Coronae		44.4	+28	28	7.2 *i*
S Hydrae		48.4	+ 3	27	10.4 *i*	X Coronae		45.2	+36	35	11.7*d*
T Hydrae		50.8	− 8	46	10.3 *i*	R Serpentis		46.1	+15	26	12.8*d*
T Cancri		51.0	+20	14	9.0	V Coronae		46.0	+39	52	7.7 *i*
S Pyxidis	9	0.7	−24	41	<13	R Librae		47.9	−15	56	12.0
W Cancri		4.0	+25	39	10.6*d*	RR Librae		50.6	−18	1	12.8
X Hydrae		30.7	−14	15	11.3 *i*	Z Coronae		52.2	+29	32	11.2
Y Draconis		31.1	+78	18	10.0 *i*	RZ Scorpii		58.6	−23	50	9.5
R Leo. Min.		39.6	+34	58	9.8*d*	Z Scorpii	16	0.1	−21	28	10.5
RR Hydrae		40.4	−23	34	<13	R Herculis		1.7	+18	38	9.0
R Leonis		42.2	+11	54	9.8*d*	U Serpentis		2.5	+10	12	10.5 *i*
Y Hydrae		46.4	−22	33	7.3*d*	X Scorpii		2.7	−21	16	11.8
V Leonis		54.5	+21	44	12.8*d*	W Scorpii		5.9	−19	53	<14
R Urs. Maj.	10	37.6	+69	18	9.0 *i*	RX Scorpii		5.9	−24	38	10.3
V Hydrae		46.8	−20	43	8.3	RU Herculis		6.0	+25	20	9.0
W Leonis		48.4	−14	15	13.0	R Scorpii		11.7	−22	42	<14
S Leonis	11	5.7	+ 6	0	10.4 *i*	S Scorpii		11.7	−22	39	12.1*d*
RX Virginis		59.6	− 5	13	8.0	W Coronae		11.8	+38	3	10.6 *i*
R Comae		59.1	+19	20	<14	U Herculis		21.4	+19	7	12.0
RW Virginis	12	2.1	− 6	12	7.4	SS Herculis		28.0	+ 7	3	10.5 *i*
T Virginis		9.5	− 5	29	13.4*d*	W Herculis		31.7	+37	32	8.8 *i*
R Corvi		14.4	−18	42	7.6 *i*	R Draconis		32.4	+66	58	7.5
SS Virginis		20.1	+ 1	19	7.2	S Herculis		47.4	+15	7	12.5*d*
T Can. Ven.		25.2	+32	3	10.4*d*	RV Herculis		56.8	+31	22	13.0
Y Virginis		28.7	− 3	52	9.8 *i*	T Draconis	17	54.8	+58	14	9.5*d*
T Urs. Maj.		31.8	+60	2	7.4	— Draconis		54.7	+54	13	11.3
R Virginis		33.4	+ 7	32	8.6*d*	V Draconis		56.3	+54	53	<14

Approximate Magnitudes of Variable Stars on Mar. 1, 1910—Con.

Name.	R. A. 1900. h	m	Decl. 1900. °	′	Magn.	Name.	R. A. 1900. h	m	Decl. 1900. °	′	Magn.
T Herculis	18	5.3	+31	0	8.0	T Cephei	21	8.2	+68	5	9.8
W Draconis		5.4	+65	56	14.0	S Cephei		36.5	+78	10	10.2*d*
X Draconis		6.8	+66	8	11.5*d*	RU Cygni		37.3	+53	52	7.8 *i*
U Draconis	19	9.9	+67	7	9.5 *i*	RR Pegasi		40.0	+24	33	12.2*d*
R Cygni		34.1	+49	58	12.5*d*	S Lacertae	22	24.6	+39	48	13.2*d*
Z Cygni		58.6	+49	46	8.3 *i*	R Lacertae		38.8	+41	51	8 5 *i*
S Cygni	20	3.4	+57	42	14.5*d*	R Pegasi	23	1.6	+10	0	11.6*d*
RS Cygni		9.8	+38	28	10.0*d*	V Cassiop.		7.4	+59	8	9.0 *i*
U Cygni		16.5	+47	35	10.0*d*	Z Cassiop.		39.7	+56	2	13.0*d*
RW Cygni		25.2	+39	39	9.8	RR Cassiop.		50.7	+53	8	11.0*d*
ST Cygni		29.9	+54	38	10.5 *i*	R Cassiop.		53.3	+50	50	7.2*d*
V Cygni		38.1	+47	47	10.0 *i*	Z Pegasi		55.0	+25	21	11.3*d*
RZ Cygni		48.5	+46	59	12.0*d*	Y Cassiop.		58.2	+55	7	<14
X Cephei	21	3.6	+82	40	10.6*d*						

The letter *i* denotes that the light is increasing; the letter *d*, that the light is decreasing; the sign <, that the variable is fainter than the appended magnitude. The above magnitudes have been compiled by Mr. Leon Campbell of the Harvard College Observatory from observations made at the Vassar, Mt. Holyoke, Swartz, Olcott and Harvard Observatories.

GENERAL NOTES.

Total Eclipse of the Sun May 8. This will be visible as a partial eclipse in Australia and the islands in that vicinity. The path of totality starts in the Antarctic Ocean and curves around south of Australia touching land only on the southern half of the island of Tasmania. The eclipse begins May 8 $15^h\ 38^m.4$ and ends May 8 $19^h\ 46^m.4$ Greenwich mean time.

Total Eclipse of the Moon May 23. This will be visible throughout North and South America excepting in Alaska. The beginning will be visible also in Africa and the southwestern part of Europe. The Moon touches the penumbra of the Earth's shadow first at $14^h\ 32^m.5$ and enters the umbra at $15^h\ 46^m.4$. Total eclipse begins at $17^h\ 09^m.0$ and ends at $17^h\ 59^m.6$. The Moon leaves the shadow at $19^h\ 22^m.3$ and the penumbra at $20^h\ 36^m.2$ Greenwich mean time. The Moon passes through the southern part of the Earth's shadow, being darkened first at a point 84° around toward the east from the north point and losing the shadow finally at 49° to the west from the north point of the disk.

Opposition of Eros in 1910.—In the Harvard College Observatory circular 153 is given an ephemeris of the asteroid Eros (433) for the opposition of 1910, when the asteroid will be of about magnitude 10.4 at its brightest. Professor Pickering says: "The orbit of Eros (433) is so remarkable, that frequent measures of its position should be obtained. Its light also varies according to peculiar and unknown laws and, therefore, measures of its brightness should be made at short intervals. Although it comes to opposition on May 23, 1910, no ephemeris of it appears to have been published. Accordingly Professor Wendell has computed the ephemeris given in Table I, which furnishes the position of Eros during the entire time that it can be conveniently observed, from before the first stationary point until after the second stationary point."

Date 1910	R.A. 1875 h m	Decl. 1875 ° ′	log *r*	log Δ	Magn.
Mar. 19	16 35.5	−42 38	0.137	9.867	10.62
Apr. 8	54.8	46 27	0.157	9.824	10.50
18	53.7	47 52	0 166	9.803	10.45
28	44.2	48 42	0.175	9.785	10.40
May 8	27.2	48 40	0.184	9.773	10.39
18	16 05.7	47 28	0.192	9.772	10.42
23	15 54.9	46 25	0.196	9.775	10.46
28	44.8	45 07	0.200	9.782	10.51
June 7	28.4	42 01	0 207	9.805	10.66
17	18.7	38 42	0.214	9.838	10.86
27	15.5	35 36	0.220	9.878	11.09
July 7	17.7	32 59	0.225	9.922	11.34
Aug. 6	15 47.3	−28 05	0.238	0.053	12.06

Water Vapor on Mars. Quotation from C. G. Abbot, "A Shelter for Observers on Mount Whitney." Smithsonian Miscellaneous Collections, Quarterly Issue, Vol. 5, Part 4 (page 506): "The observations of Director Campbell on the spectrum of Mars were entirely conclusive in showing that water-vapor, if present at all in the atmosphere of Mars, is in less quantity than is contained in the extremely rare and dry part of the Earth's atmosphere which is above Mount Whitney. In fact, no evidence at all of water vapor on Mars was detected by Campbell."

"Unfortunately, both Director Campbell and myself were on Mount Whitney during unusually unfavorable weather, for the whole southwest, including northern Mexico, was just at that time visited by floods of rain and cloudy weather. Such a condition would not probably be met with at that season one year in ten."

This admission speaks for itself. The excessive moisture must have pervaded the air generally to the masking of moisture on Mars. Even ordinarily summer is the most unfavorable time for getting any results, because the Earth's moisture is then at a maximum.

Lowell Observatory,
Supplement to Bulletin No. 43.

The Largest Telescope in the World. A reader of POPULAR ASTRONOMY sends us the clipping, given below, from a Montreal paper and asks if the statements are true. He says "I had supposed that the object glass alone governs the magnifying power." The title is misleading but the statements made in the article are probably correct. The magnifying power of a telescope depends not upon the diameter of the object glass but upon the ratio of its focal length to that of the eyepiece. The greater magnifying power, however, is available only upon bright objects like the Sun, Moon and planets and in favorable conditions of atmosphere. For faint stars, clusters and nebulae the Lick and Yerkes telescopes having greater light-gathering power should be more effective than the Treptow telescope. The latter too will be practically useless for scientific measurements when the wind blows.

"Treptow Observatory, near Berlin, now boasts the most powerful telescope in the world. Dr. Frederic S. Archenhold, director of the observatory, admits that the lens of this instrument is smaller than the telescope lenses of the Yerkes and Lick observatories, but, in spite of that, it has a magnifying power greater than any yet built.

The Yerkes lens has a diameter of 104 centimeters, the Lick lens 96 centimeters, while the Treptow instrument has a lens of only 70 centimeters in breadth. The Lick telescope magnifies 4,500-fold, while the Archenhold telescope has a power of 6,000 diameters under favorable atmospheric and light conditions. This increase of a power with a smaller lens is explained by Professor Archenhold as possible through a greater focal distance. Thus the length of the Yerkes telescope is 18 meters, that of the Lick 15, while the Archenhold is 21 meters.

The lens is of extraordinary fine workmanship and cost $12,500. It was cast in Jena by Professor Abbe and Dr. Schott. Dr. Steinheil of Munich was responsible for the delicate work of cutting it. The metal tube of the telescope proper is inclosed in an outer metal covering, so that the temperature may be more uniform and the instrument protected from vibrations caused by the movements of the atmosphere. The apparatus weighs 5,500 pounds, and is operated by electricity.

The distinguishing feature of Treptow Observatory is that it has no revolving cupola. The advantages of the cupola are obtained through the outer metal enclosure of the telescope, and the cost of the observatory is greatly reduced by not having the great revolving chamber usual in observatories.

The observatory has many original features, none of which have been patented, so that astronomers are free to use them without the payment of royalties. Andrew Carnegie is one of the wealthy men who has given donations to make the Treptow Observatory one of the best equipped scientific establishments in the world."

Halley. In view of the return of Halley's comet, which has now been found by the telescope, some account of the great astronomer with whose name the comet is associated may prove of interest to the general reader.

The great French astronomer Lalande considered Halley to be the greatest English astronomer of his time, and this opinion is certainly just. He was doubtless one of the greatest men of science that England, or indeed, any country, has ever produced.

Edmond Halley was born in London on November 8, 1656. From his earliest year he applied himself with zeal to the study of mathematics and astronomy, and obtaining such instruments as his means permitted he studied the heavens, and took advantage of every fine night to improve his knowledge of astronomy.

Knowing that a considerable amount of excellent work had been done on the solar system and among the stars of the northern hemisphere by Cassini, Flamsteed, and Hevelius, Halley thought it better not to enter into competition with these great observers, and decided to turn his attention to the southern stars of which no good observations had then been made. It was reported that a Dutch observer named Houtman had observed these stars in the island of Sumatra, and that Blaeu, the globe maker, had used these observations in the correction of his celestial globes. But Halley, on examining these corrections, came to an unfavorable opinion about the observer and his instruments. Having consulted with some of his friends on the best station to choose for his proposed observations of the southern stars, Halley decided on St. Helena. His father, a citizen of London, promised to supply him with the money necessary for the purpose. He was recommended to King Charles II by Williamson

and Jones Moore, and the Indian Company, who had control of the island, promised him all the assistance he required.

Having decided on the undertaking, Halley had a sextant of 5½ feet constructed, somewhat similar to that of Flamsteed, and he procured telescopes and micrometers. The largest of the telescopes was 24 feet in length. He sailed for St. Helena in November, 1676, at the early age of 20, and after a satisfactory voyage of three months he arrived at the island.

Although he had heard good accounts of the climate, it proved very disappointing. Frequent rains and a constantly hazy sky hardly permitted any observations in the months of August and September, and although he lost no opportunity for observation he found that at the end of the first year he had only observed some 360 stars.

Notwithstanding these difficulties he consoled himself with the idea that his labors would not be wholly in vain, and he connected the stars he observed with others whose positions had been determined by Tycho Brahé. His catalogue of southern stars, reduced to the epoch of 1677, was published in London in 1679.

In forming his catalogue he had necessarily to identify the stars he observed, and in this connection he made the following remarks with reference to stars in the constellation Sagittarius: "It may appear strange that the stars which are before the left hock and the knees (in the ancient figure of a centaur), which Ptolemy called bright and of the second magnitude, are at present of the fourth at most. This appears to show if not corruptibility, at least the changeability of the celestial bodies. This diminution of light may be attributed to some accident. It is probable that for a similar reason the stars of the left leg and right knee are now so small that Tycho overlooked them." Halley further remarks that some stars which Ptolemy placed before (that is, west of) Piscis Australis, and of which four are noted of the third magnitude, are at present only of the fifth or sixth, and he asks if this is due to the diminution of their light in the lapse of time.

In addition to his work on the stars, Halley made some investigations on the Moon's parallax, combining his observations at St. Helena with those made in Europe. He also made some researches on the theory of the Moon's motion, which was not thoroughly understood in his time, and which were so necessary for the determination of longitudes.

On November 7, 1677, Halley observed a transit of Mercury across the disk of the Sun. From his observations, which were not, however, obtained under favorable conditions, he derived a value of the Sun's parallax of 45″, which is, of course, much too large, the real value being about 8″.8. But transits of Mercury are not suitable for measures of solar parallax, and, indeed, Halley pointed out that transits of Venus are much better for the purpose.

In 1679, Halley paid a visit to Hevelius to satisfy himself as to the accuracy of observation which the latter had claimed for his method of pinnules without the aid of a telescope. Halley convinced himself that the error of the observations made by Hevelius were less than had been supposed, and did not exceed a minute of arc.

In the year 1684 Halley paid a visit to his friend, Sir Isaac Newton, to confer with him about the law of gravitation which had been previously discovered by Newton. The result of the consultation between these great men led to the publication of Newton's famous work, the *Principia*. The first book of this immortal work was presented to the Royal Society of London in April, 1686, and ordered for publication in May of that year. But as the finances of the Society

would not admit of paying for the printing of the work the expense was generously defrayed by Halley. The *Principia* was published in 1687, and it is pleasant to know that Halley's expenses connected with its publication were afterwards refunded by the sale of copies of the work.

Halley wrote a work on comets. Among former writers on the subject he found that the first account of a comet of any use for his purpose was one by Nicephorus Gregoras, who in the year 1337 recorded carefully the apparent path of a comet in the sky. But he was not so accurate in his record of *time*, and it was only on account of its having dated back by over 300 years that Halley included it in his catalogue of comets. The great comet of 1472, which made a near approach to the Earth, was observed by Regiomontanus. This comet described an arc of 40° in one day, and is the first of which satisfactory observations were made. The comet of 1577 was well observed by Tycho Brahé. Kepler observed two comets, and from their parallax he concluded that comets pass freely through the planetary orbits along a path which differs but little from a straight line. These views were supported by Hevelius, but Borelli seems to have thought that comets moved in elliptical or parabolic orbits round the Sun, and this theory was confirmed by the great comet of 1680, which the observations of Halley and Newton showed to be moving in a curved path round the Sun.

In his researches on the motions of comets, of which he computed the orbits of 24, Halley noticed that those recorded by Appian in 1531, by Kepler in 1607, and by Halley himself in 1682, seemed to return after a period of 75 or 76 years. He thus suspected that these comets were one and the same. This suspicion seemed to be confirmed by the recorded appearance of bright comets in the years 1456, 1380, and 1305, the intervals again being 75 or 76 years. He rightly conjectured that the comparatively small differences in the recorded intervals were due to the disturbing action of the larger planets. This was especially so when he found that in the year 1681 the comet passed near the planet Jupiter, the attraction of which must have had a considerable influence on the comet's motion. Making due allowance for this disturbing influence of Jupiter, he computed that the comet would return to the Sun's vicinity about the end of 1758 or beginning of 1759. Halley did not live to see his prediction fulfilled (he died in 1742), but the comet returned, and, according to the calculations of Clairaut, its nearest approach to the Sun was due on April 13, 1759, with a possible error of one month. The comet actually passed through perihelion on March 13, 1759, only one month earlier than the date fixed by Clairaut! This showed the accuracy of Halley's foresight, and the comet has ever since been known by his name, although he was not its original discoverer. The comet again duly returned in 1835, and its next passage through perihelion will take place about April 20, 1910. This is now certain, as the comet was found on a photographic plate taken by Dr. Max Wolf on the morning of September 12, 1909, within a few minutes of arc of the position predicted by the calculations of Messrs. Cowell and Crommelin of the Greenwich Observatory—a great triumph for astronomy and mathematics.

In addition to his merits as an astronomer, Halley was a good navigator. In 1698 he obtained command of a vessel and voyaged in the Atlantic Ocean for the purpose of making observations on the laws which govern magnetic variation. He returned to England in June, 1699. His lieutenant, who had given him much trouble, was dismissed, and Halley continued his voyage. He reached 52° of southern latitude, when the ice compelled him to turn back, and after some adventures he reached London on September 7, 1700. He had another

voyage in 1701 and 1702, and then retired from travelling.

In November, 1703, Halley succeeded Wallis as Savilian Professor of Geometry at Oxford. In 1713 he was elected secretary of the Royal Society, and in 1721 he succeeded Flamsteed as Astronomer Royal at Greenwich. He was then 64 years of age, but still full of zeal and energy. When Halley took charge of the Greenwich Observatory, he found it almost devoid of instruments, Flamsteed's executors having removed all those which belonged to him. He obtained a meridian circle, similar to that of Roemer, which he used for four years to observe difference of right ascension. In 1725 a quadrant of five feet radius was set up in the meridian, and his efforts were chiefly directed towards the improvement of the lunar tables for the determination of the longitude at sea.

Halley observed the great aurora, which occurred on March 16, 1715, and his accurate prediction of the total eclipse of the Sun on May 2, 1715, added much to his reputation.

He died on January 14, 1742, at the good old age of 85.

Knowledge and Scientific News, Nov., 1909. J. E. GORE.

Another Who Saw Halley's Comet in 1835. When Halley's comet appeared in 1835, I was in my 12th year; and during that year there was a total eclipse of the Sun. Every part of his disk was covered. This occurred on Sunday, about noon, either in May or June, stars became visible, fowls began to go to roost; and all nature looked weird and desolate, and in my ignorance of such things, I thought the end of time was at hand, and expected to hear the Trump of Gabriel proclaim that time was no longer. During the same year, on one Saturday afternoon about three o'clock, the sky was murky, and the Sun, without his bright rays, could be seen, as we look at the Moon. I saw what I thought to be a buzzard flying across his disk. I watched it for a few minutes, and as it did not move as I expected, I hastened to the house to report the strange sight. To me it appeared to cover, as well as I can now remember, at least one-twentieth of his whole disk. During the same year I saw what seemed to be a ball of fire start from about 45° in the northwest and watched it disappear below the horizon; and a loud report like the firing of a cannon was heard.

On the 13th of November, 1833, after night the whole heavens seemed to be aglow with shooting meteors. Several times since that date, I have seen similar showers of meteors on the 13th of November, but nothing like the display of 1833. I remember well the comet of 1843.

Ackerman, Miss., Feb. 12, 1910. H. P. DOTSON.

Journal of the Royal Astronomical Society of Canada. The January-February number of the *Journal* of the Royal Astronomical Society of Canada contains two very interesting and valuable papers: one the annual address of the retiring president, W. Balfour Musson on "Recent Discussion in Astronomy and Astrophysics"; the other entitled "Scientific Crumbs from Europe" by Otto Klotz. Four plates give three fairly good reproductions of Comet *a* 1910 taken on the dates Jan. 25, 28, and 31, and one of Halley's comet taken on Feb. 10, by R. M. Motherwell of the Dominion Astronomical Observatory.